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THE PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,
THE LIBERAL AND FINE ARTS,
GEOLOGY,
AGRICULTURE,
MANUFACTURES AND COMMERCE.

NUMBER CLXXXIX.
For JANUARY 1814.

CONTAINING THE FOLLOWING ENGRAVING:
A Plate to illustrate a New Transit Instrument invented by Sir H. C. ENGLEFIELD, Bart.

By W. NICHOLSON AND A. TILLOCH.

LONDON:
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ADVERTISEMENT.

From the commencement of this work, it has been our aim to lay before our readers every thing new or curious in the objects embraced by our plan; and aided by the contributions of numerous respectable correspondents, to whom we are under many obligations, we have been enabled to present an instructive and useful miscellany.

We have now to congratulate our readers on the cessation of war—the enemy of philosophical intercourse. For a long period it has been with difficulty that even foreign *Transactions* and *Journals* could be obtained: indeed the publication of several of them had been entirely suspended. As to regular intercourse with the conductors of the continental periodical works, with many of whom we were years ago personally acquainted, and in habits of regular communication, it had, for a considerable time, entirely ceased. The return of peace has already renewed the correspondence; and without subjecting ourselves to the imputation of boasting, we may be permitted to say, that the readers of the *Philosophical Magazine and Journal* will reap increased advantages from this source of information, as well as from the revived continental journals.

With the last number of this volume two title-pages are given. It was intended to continue this practice, but on more mature consideration the plan has been altered. The references to future volumes could not, without confusion and the risque of many errors, be adapted to a two-fold numeration; and therefore, to bring *The Philosophical Journal* at once into series with *The Philosophical Magazine*, the extra title-page is adapted to serve for Vol. XXXVII. to XLIII. of *The Journal*; and those who are possessed of that work are requested to give directions to the binder to letter the back of the volume accordingly. When the present volume may be referred to in future numbers, it will be quoted as Vol. XLIII.

Gentlemen whose sets of the Philosophical Magazine are defective, should make an early application for the numbers that may be wanting, as many of them are getting out of print.

London, June 30, 1814.

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CONTENTS.

THE

THE

PHILOSOPHICAL MAGAZINE

AND JOURNAL.

I. *Description of a new Transit Instrument. By Sir* H. C. ENGLEFIELD, *Bart. F.R.S. F.S.A. &c. &c. &c.*

THE transit instrument, of which I propose to give a description, was thought of in consequence of a request from Mr. Thomas Jones, astronomical, &c. &c. instrument maker, in Oxendon-street, who, having been often applied to by watch-makers, and others not much conversant with astronomy, to make for them a cheap, simple, and easily placed instrument, for the purpose of obtaining time with accuracy, requested me to assist him with my ideas on the subject.

An old transit instrument formerly belonging to Mr. Aubert, and at his sale purchased by Mr. Walker, gave me the first hint for the instrument now to be described. It had, like this, its telescope in the axis; but the reflecting mirror was between the object- and eye-glasses, which rendered it cumbersome, and liable to many inconveniencies, which are, I think, avoided in the mode of construction now adopted.

It may be here observed, that Hevelius, in his Selenographia, describes a telescope in which a mirror is interposed between the object-glass and eye-piece, thereby enabling an observer, without danger, to look out of a besieged place, the telescope being placed vertically under shelter of the rampart. It seems singular that he should not have perceived how much more commodious and simple his instrument would have been, had the mirror been placed beyond the object-glass, and made so as to turn all round, as it has been of late years made to do in some of the large camera obscuras.

To return to the present subject. It is well known that the transit instrument, in its usual form, is liable to great injuries from blows, or other violence affecting the perpendicularity of the telescope to the axis; that it necessarily takes up much room in package; that, unless of a very small size, it is not easily

fixed in a window or other opening in a common dwelling-house; and that it is quite impossible, excepting in fixed observations, to make it sweep the entire arch between the southern and northern points of the horizon. In windy weather its use is also very difficult and inconvenient. In the instrument now offered, all these inconveniencies are avoided. It may be fixed almost any where; in many places it may be made to describe the entire semicircle of the meridian; the observer is put to no difficulties by change of place, as he always looks directly along the axis; it is packed in one eighth of the space requisite to pack a common transit instrument of the same real size; its weight is not more than a sixth of the other; from its simplicity it will be afforded at half the price; and its verifications and adjustments are easy and simple. It has also another advantage—that the mark by which it is placed in the meridian may be either in the meridian or at right angles to it; or, if convenient, two marks may be erected, one to the south or north, the other east or west; and if so used, it will be always seen by inspection only, whether the mirror needs adjustment or not. In many confined situations, such as occur in cities, the power of having a mark at right angles to the meridian may be eminently useful. The general description of the instrument is as follows:

The telescope is included in a brass cylinder having a small cylinder at each end, turned true in the usual manner, and resting in Ys of the usual construction. These smaller cylinders are both pierced. In one is the eye-piece of the telescope with its wires, &c. The other is open for the purpose of seeing through it, if necessary, the eastern or western mark, and for adjusting by direct vision the line of collimation of the instrument. It also serves for the illumination of the wires. The object-glass of the telescope is placed so near this cylinder as only to allow room for an unsilvered plane glass mirror to be placed before it, at an angle of 45°. It is obvious that, as the telescope revolves on its axis in the Ys, every celestial object at right angles to it will successively be seen by reflection from the mirror; and, of course, if the axis be placed due east and west, the transits of all celestial bodies over the meridian will be observed with the utmost accuracy and convenience. The aperture in the axis beyond the object-glass is not only of use for the adjustment of the instrument to an eastern or western mark, or for the illumination of the wires, but affords a means of seeing the mark at the same time with the body whose meridian passage is to be observed, and of thereby being certain of the true adjustments of the instrument at the very moment of observation; which is impossible in any other construction of the transit instrument, and seems to be a very material advantage.

I am

I am convinced that transit instruments of the largest size might with very great advantage be constructed on this principle. It is true that where very much light is wanted, as in observations of stars in the day-time, the loss by reflection will be some disadvantage; but the loss of light from an unsilvered mirror is very small; and the convenient and simple form of the instrument, by its lightness, less subject to flexure; by its position much less liable to errors from unequal change of temperature; and so extremely commodious in its use; present advantages of a very important nature, and such as might introduce it into the most extensive observatories with profit. The advantages may perhaps be even greater than in small transit instruments. When in the present construction the telescope is long, as its whole weight rests on the most disadvantageous point of the axis, this is of necessity made very large towards its centre to avoid flexure, the whole instrument so heavy that an additional apparatus of counterpoise must be added to the pivots, lest they should wear away the Ys, and the reversing the instrument becomes a work of some difficulty and danger: whereas in the construction proposed, the small comparative weight of the object- and eye-glasses lies very near the pivots, and the middle of the tube is the lightest part of it. The operation of reversing is performed with great ease, both from the form and lightness of the instrument; and it may be added, that the comparative facility of observations is of greater advantage the larger the instrument.

Having so far explained the general principles of this transit telescope, which was executed very much to my satisfaction by Mr. Thomas Jones, it will be proper to describe more particularly its construction.

The transit stand is represented by either fig. 1 or 2 (Pl. I). The first is constructed for being placed or fixed on a vertical surface; the second for a horizontal one, A, B; in both are the Ys, or supports for receiving the axis of the telescope. The end A intended to be always next the eye is furnished with both the horizontal and vertical adjustments, such as are usual to transit instruments. The telescope axis is represented by fig. 3. The eye end being at C, the object end at D turns round in the Ys AB upon its cylinders *n*, *n*; the screw head *r*, at the object end D, is for the purpose of adjusting the parallel glass. The telescope is adjusted to distinct vision by means of the head or knob at P. The wires of the telescope are adjusted by means of the four capstan head-screws at the eye end *t*, *t*. The circle R is divided on the surface next to the eye. The eye-tube has a sliding motion for viewing the wires distinctly; the short piece of tube at the object end turns round on the telescope tube, and serves as a cover for the object-glass.

 Fig.

Fig. 4 is the riding level, and is placed upon the axis in the Ys, and adjusted by means of the screw S. Fig. 5 represents the stand, telescope, and level, displaying the position in which they are used. Fig. 6, for placing the transit in the meridian.

Of the verifications of this instrument two are common to every construction; one only is peculiar to this. The line of collimation is adjusted by looking direct at some distant small point (the cover being turned over the lateral aperture), and turning the telescope gradually round on its axis, and moving the screw of the wires, if necessary, till the spot is in every position covered by the intersection of the wires. The axis is brought to an horizontal position, and the level is in the same mode adjusted by reversing the telescope or level, and correcting half the error by the level screw, and half by the vertical screw of the Y in the usual mode.

The verification peculiar to this instrument is that of the mirror, and perhaps the best mode of doing this is by the pole star when nearly in an eastern or western position from the pole; its motion in azimuth is then so slow as to give ample time for the adjustment. Bring the pole star to the vertical wire (the line of collimation having been previously adjusted); then reverse the telescope in its Ys; and if the star is still on the wire, the mirror is in adjustment: if not, correct half the difference by the mirror-screw, and half by the horizontal of the Y till the error vanishes. This adjustment may also be performed by setting up a board with two parallel perpendicular lines drawn on it distant from each other, exactly the space between the positions of the mirror when the telescope is reversed in its Ys. If the vertical wire be brought to cover one of the marks, and on being reversed the wire covers the other mark, it is right. If not, the error must be made to vanish by correcting it half and half as before directed for the adjustment by the pole star.

The following method of placing the instrument correctly in the meridian, is equal if not superior to any that has yet been devised*.

Let Z, fig. 6, be the zenith; P, the pole; HO, the horizon; ZPI, the meridian circle; ZK, a circle of altitude distant from the meridian by a small quantity IK (suppose a degree); 1,2,3,4, the diurnal circle of the pole star, whose radius is 1° 45′ nearly; and let the altitude of the pole be 51° 30′. Then when the pole star is on the northern meridian its altitude 3 I will be 49° 45′, and its zenith distance Z3 = 40° 15′; and ACD be a part of the diurnal arch of a star whose polar distance is 46° 30′, and N. meridian altitude 5°.

* It had been invented by me many years ago, but has not, as far as I know, been as yet published. I therefore give it here, as not inapplicable to the subject of this paper.

Now,

Now, suppose the transit instrument, whose axis is accurately levelled, and of course in the meridian at Z, to point at the horizon to K (it is obvious from its construction that the telescopic axis will be at right angles to the meridian line) instead of I, the true meridian; then at 3 (the altitude of the pole star under the pole) it will point at B, and the arch 3 B will be to IK as the cosine of the altitude 3 I to radius; but 3 B, measured on the diurnal circle of the pole-star, will be the sine of its distance from the meridian to the radius P 3 or PB: and as, in small arches, the arch of a great circle, or of a small circle, or their sines, are nearly coincident, we shall have very nearly, As Z 3 (the zenith distance) to P 3 (the polar distance), so is the value of 3 B, in degrees of the pole-star circle, to its value in degrees of a circle whose radius is Z 3. And as the radius Z 3 is to P 3 very nearly as 23 to 1, the error of the transit telescope, at the altitude 3 I, will be measured by a scale (if it may be so called) 23 times as great as itself.

Now, let there be another star A, whose northern meridian altitude is as small as it conveniently can be; for example, 5°, whose polar distance is, therefore, 46° 30′, and whose right ascension is the same as that of the polar star; then, if the transit telescope be in the meridian, both these stars will pass through it at the same time; but if it be out of the meridian by the quantity I K, the star A will pass through it when it comes to C, but the polar star not till it comes to B, when the star A is got to D, in its diurnal circle.

The value of AC being therefore found, by multiplying IK by the cosine of its altitude AI, that value being reduced to the angular value to the radius PA, will give the time of the star A passing through the transit telescope, after the time of its passing the meridian; and the same operation being performed for the pole-star as before directed, the difference of these times will be the error in time of the transits, answering to the given deviation IK of the transit telescope. And tables having been previously constructed for such stars as shall be thought convenient, the transit telescope may, in a very short space of time, be set to the meridian, with a degree of precision unattainable by any other method.

If the star A precedes the pole-star in its passage under the pole, no tables are requisite, nor any thing necessary to be known but the exact difference of the right ascension between the two stars; for, having observed the transit of the star A (the instrument being previously brought near the meridian, suppose half a degree), then elevate the telescope to the pole-star, by moving the horizontal adjustment of the axis: keep the pole-star on the middle wire till the due interval of time between

their transits is elapsed; the instrument will then be extremely near its true position; and, by repeating the observation once more, will be brought to a perfect exactness. Or, if another star, following the pole-star in its passage, be observed on the same evening, if the times elapsed between their transits are equal to the tabular difference or their right ascensions, which will probably be the case, the accuracy of the first placing the instrument will be immediately ascertained. Other stars near the pole may be made use of in the same manner as is here described for the pole-star, but with proportionally less advantage as the polar distance is increased.

It is also obvious from the figure, that the transit of the pole-star above the pole may be also used, and that with nearly, though not quite, the same advantage as the transit below the pole.

The same method may also be applied with equal ease, if the second star A pass the southern meridian instead of the northern.

The slowness of the pole-star's motion, though it renders its transit uncertain to a few seconds, cannot materially affect the accuracy of this method, as an error of ten seconds in time, in the estimation of its passage, which is certainly more than can be committed, would not cause an error of a third of a second of time in the passage of stars near the equator.

Example of the Computation with the Numbers given above.

Star A.			Pole Star.		
Sin.	IK	8·241855	Sin.	IK	8·241855
Sin.	ZA	9·998844	Sin.	Z3	9·810316
Sin.	AC	8·240199	Sin.	3B	8·052171
Sin.	PA	—9·860562	Sin.	P3	—8·484848
Sin.	APC	8·379636	Sin.	3PB	9·567328
	APC	1° 22′ 20″		3PB	21° 40′ 10″
In time		$5^m\ 29\frac{1}{3}^s$.	In time		$1^h\ 26^m\ 40\frac{2}{3}^s$.

The error of a degree, therefore, in the position of the transit telescope at the horizon, causes the star A to pass through it $5^h\ 29\frac{1}{3}^s$ in time later than it ought; whereas, the same error causes the transit of the pole-star to be $1^h\ 26^m\ 40\frac{2}{3}^s$ later than it ought; and the difference between these two times, viz. $1^h\ 21^m\ 11\frac{1}{3}^s$, will be the difference of the observed time of their transits, owing to the error of the position of the transit telescope, their real right ascension being supposed the same.

To Messrs. Nicholson and Tilloch.

II. *On*

II. *On the French Measures and Weights.*

M. J. Brisson in his *Instruction sur les Mesures et Poids Nouveaux*, &c. Édition monotype, à Paris, An. VIII.

P. 12, states the value of the métre in the old measures to be "36 pouces 11 lignes and 296 thousandth parts of a ligne."

This measure reduced will equal .. .. pieds $3{\cdot}078\dot{4}$*

For 36 (Pouces) 11·296 (Lignes) = 443·296 (Lignes) ÷ 12 = $36{\cdot}941\dot{3}$ (Pouces) ÷ 12 =
as above .. .. pieds $3{\cdot}078\dot{4}$*

In p. 26, he states the value of the métre to be "exactement" .. .. pieds 3·078444

Ibid. He states the value of the décimètre to be "exactement" .. .. .. lignes 44·3296*

Now 44·3296 (Lignes) ÷ 12 = $3{\cdot}6941\dot{3}$ (Pouces) ÷ 12 = $0{\cdot}3078\dot{4}$ (Pieds in a decim.) ×
10· = for 1 métre .. .. .. pieds $3{\cdot}078\dot{4}$*

From the above statements it is evident that the métre, as deduced from the pied in page 26, being .. .. = $3{\cdot}078\dot{4}$*

and as deduced from the lignes in the same page being .. .. = 3·078444

They differ ... $0{\cdot}000000\dot{4}$

parts of a pied, and therefore cannot be "exactement" alike, which they should be; and this discrepancy is troublesome in the verification of calculations.

In p. 17, he states the centiare or métre quarré to .. .. .. = pieds quarrés 9·476817.

Now if the métre be $3{\cdot}078\dot{4}$ (Pieds), the métre quarré will be .. .. .. $\left.\begin{array}{r}9{\cdot}476820\\ \dot{1}9753086\dot{4}\end{array}\right\}$*

and if the métre be 3·078444, the métre quarré will be .. .. .. $\left.\begin{array}{r}9{\cdot}476817\\ 461136\end{array}\right\}$

Both which, it is evident, differ from his centiare or métre quarré above.

Ibid. He states the litre ou décimètre cube to = pouces cubes .. .. 50·412416.

Now if the métre be $3{\cdot}078\dot{4}$*

the décimètre will be $\cdot3078\dot{4}$ and $\cdot3078\dot{4}^3 = 50{\cdot}4124378$,&c.*

And if the décimètre be ·3078444 $\cdot3078444^3 = 50{\cdot}4124160008$, &c.

But

But this last value of the litre, although it agrees very nearly indeed with M. Brisson's statement of its value, in page 17 of his Instruction, is the least correct of the two, supposing the Report, stated in the third volume, 4to, p. 324 of Nicholson's Journal, as made to the National Institute, that the "true and definitive métre is 443·296 lignes," be correct; and in that case all the calculations marked with a star (*) above are correct, and the others erroneous. H. G.

To Messrs. Nicholson and Tilloch.

III. *On the Treatment of Burns and Scalds.* By Mr. Richard Walker, *Surgeon and Apothecary, Oxford.*

Oxford, Jan. 6, 1814.

It is a matter of no ordinary importance, in the profession of surgery, to know the best mode of treating *burns and scalds*, particularly in what manner to proceed immediately after the injury has been received, and which it seems is still a *desideratum*.

It is singular, that even of late years, two modes of practice, directly opposite to each other in principle, have been recommended by practitioners of the first eminence in the profession.

One method, and I believe the most prevalent one, is to counteract or prevent the ill consequences arising from a *burn or scald*, as in other instances of inflammation, by an *antiphlogistic process;* viz. by *cold sedative applications:* the other, which is, the latest, is by the adoption of a *stimulating process;* viz. by the use of *inflammant stimulating* applications, and these previously made *warm* or *rather hot.*

The abettors of each method give a *rationale* of the means by which each mode produces its peculiar good effect.

The former of these methods is so consonant with common opinion, and the ordinary mode of reasoning, as to require from me neither explanation nor comment.

The latter mode, however, is, at first view, so repugnant to ordinary reasoning, as to excite attention.

The *rationale* upon which this is stated to act, is by counteracting or preventing ensuing *inflammation, vesication*, and the other ill consequences which are incident to injuries of this nature; that is, by not suffering the *excitement, irritation*, or *increased action*, in the part, produced by the injury, to *subside too hastily;* but, by the immediate application of *appropriate stimuli*, to maintain this increased action for a certain time; and afterwards, by a due adjustment of the *succeeding stimuli*, to diminish it *gradually*, until the part injured is by these regulating means,

means, in due time, suffered to regain its natural or ordinary state.

It is proper to observe, that the author of this process entertains a decided opinion of the superiority of his own plàn, in preference to the *antiphlogistic one;* and that this opinion is sanctioned at present, I believe, by the practice of many gentlemen of eminence in the profession.

The *process of the stimulating plan,* recommended and pursued by Mr. Kentish, is doubtless well known, being described in the various professional works, and therefore an account of it here is unnecessary.

With respect to the theory, of which I have only given the leading principle, by which the good effect of the *stimulating plan,* in *burns and scalds,* is supported by Mr. K. I must confess it appears to me, as it does to some other professional persons, remote and unsatisfactory; but, after all, it must be admitted that *facts* alone are to be relied on.

Had this latter method been introduced into practice, at the time I had an opportunity of making observations at the Radcliffe Infirmary, I might probably have been prepared to have delivered an opinion respecting its comparative efficacy in point of *fact.*

In private practice, having good reason to be perfectly satisfied with the plan here recommended, I have judged it imprudent to adopt so bold, and, in my opinion, so indefensible a mode of practice, upon the authority of any person: therefore I have hitherto never tried it.

For my own part, I consider *cold,* as the direct and proper antidote for *heat;* and upon this principle, chiefly, I think the *immediate* and *remote* ill consequences of a *burn* or *scald* are to be obviated; and amidst the various methods I have witnessed the trial of, in cases of this nature, no other mode of treatment has answered so well as the method I am now about to mention.

In consequence of my observations, on cases of this kind, which occurred at the Radcliffe Infirmary, during a period of nearly five-and-twenty years, I collected that the following mode of treatment was the most successful; and, accordingly, I employed it there, in those cases in which I had an opportunity, and which, with some slight variations, I still pursue.

In the first place, I apply a dressing of the *simplest emollient cerate*; viz. one composed of *wax and oil,* spread rather thick, upon soft old linen of a *close texture,* in order to the exclusion of the atmospherical air; an exposure to which, under these circumstances, I consider to be extremely injurious; and immediately after, apply compresses soaked and wet with the *liquor*

plumbi

plumbi acetatis dilutus, and as cold as may be, renewing or continuing this process, by means of two compresses, one of which remains in the cold liquor, whilst the other is applied so often, and so long, as the part injured is *above the natural temperature;* that is, until the heat and irritation arising immediately from the injury have subsided, and which, by this means, duly pursued, is usually accomplished in a few hours.

In the instances of *scalds*, where the injury is only superficial, the same method is persevered in, renewing the dressing or pledget of cerate once or twice every four-and-twenty hours, according as the discharge, pain, or other circumstances may indicate; still continuing the cold application, especially toward the afternoon or evening, when the part is more inclined to become hot and painful.

Whatever *vesications* might ensue, I immediately emptied of the effused serum, by as small a puncture as might be, and pressed the whole of the fluid gently out, preserving the *cuticle*, till it separated spontaneously, or the denuded part was become callous, taking particular care, at the time of removing it, and likewise at every time of dressing, to cover the part, as expeditiously as possible, in order to exclude the external air.—Particular care, likewise, should be taken that the part be well moistened or wetted, with the liquor before mentioned, previously to removing the plaster, and compress, in order to prevent their sticking or adhering to the part.

In *burns or scalds* of a more serious nature, where *sloughs* are likely to form, the second part of the process consists in applying a *cataplasm of bread*, and the *saturnine liquor* prepared as before, and *cold*, if the heat of the part require it; repeating it twice, or oftener, every four-and-twenty hours, according to circumstances. It is scarcely necessary to state, that rest in bed, unless in trivial cases, is essential, with an *antiphlogistic regimen*, and *opium* occasionally.

In cases of such a nature as to threaten *mortification*, or *the loss of life*, care must be taken that the *cooling antiphlogistic system* be not pushed too far, that is, beyond what the habit and strength of the patient can endure; which will be apparent, by the want of a due degree of *reaction* in the part injured, or in the system in general; in which case the use of *warm fomentations*, and *emollient* or *stimulating cataplasms*, according to circumstances, should be resorted to; and a due portion of invigorating nourishment and cordials, as wine, &c. thrown into the habit. I am of opinion that *bark* is of little use in such cases.

I was led to the above method of treating *burns* and *scalds*, in consequence

consequence of having observed, in many cases, the effects of *emollient applications*, and of the *cold saturnine lotion*, used both separately and conjointly, to various cases of this nature—and likewise from the effects of the stimulating applications, then in fashion with some practitioners, viz. *vinegar, camphorated spirit of wine*, &c.

I have two cases of *severe scalds* at this time (July 12, 1812) under my care; one which has been dressed in the ordinary way with *emollient cerate only;* the person neglecting to apply the *cold lotion* as I had directed.

In the other case, both legs were severely scalded, from the feet to the knees nearly.

In this instance the patient very prudently, in my opinion, immersed his legs instantly, without waiting to take his shoes and stockings off, in a tub of cold water, which happened to be near, for some time, and then applied to me. I pursued the plan before mentioned; the next day there were several *vesications* of considerable size, (although the greater part escaped this effect,) which I immediately punctured, pressing out the effused serum. At a week's end from the time of the accident, the denuded parts, and those which were still covered in part with the detached *cuticle*, were entirely free from inflammation and pain, and scarcely any tenderness remained; and in less than a fortnight from the time of the accident, he was perfectly well; no suppuration or discharge whatever having succeeded to the accident. The cold lotion was applied by this person, almost unremittingly, night and day.

In the former instance, the blistered parts suppurated, and were many weeks in healing.

The ordinary progress, as I have found by repeated instances of cases treated *assiduously* in the manner recommended above, has been according to the event in that instance; and in cases of *burns* or severer *scalds,* in which *sloughs* have formed, the event, by following the directions before given for the treatment of such cases, has been equally successful, allowing for the greater degree of injury received.

I think it unnecessary, and even injurious, to use any application *colder*, either in winter or summer, than that temperature at which spring water usually is all the year round; viz. about 50° of Fahrenheit, (which temperature of water, as is well known, may be obtained throughout the year, from a pump, a pailful or two having been previously pumped off;) and the more frequently this is renewed, especially at first, the better:—pumping upon the part, however, by its mechanical force, might be injurious.

After the first or second dressing, I prefer the use of some *desiccative cerate,* to the emollient one; viz. a *saturnine cerate,* of

of which the following is a good *formula*. Take of olive oil and white wax, each sixteen parts; white ceruse two parts, and Goulard's extract of saturn (liquor plumbi æcetatis) one part, mixed:—in winter, the proportion of the wax should be only fourteen parts*:—the proportions of each article are meant by weight.

My intention here is to confine myself entirely to the result of my own experience *alone*. Therefore, with respect to the *comparative effects* of this mode with the one lately recommended by Mr. Kentish, in injuries of this nature, I can say nothing, having never witnessed myself the effect of it. But perhaps it may not be going too far to state, that the mode of treatment recommended by that gentleman has been used here by professional persons, who have again returned to the *cooling antiphlogistic mode* of treatment.

It has been the practice of some practitioners, to push the *cold plan* so far, as to use even ice and snow to *burns* and *scalds*: of this, likewise, I can say nothing from my own experience.

In *burns* or *scalds*, of such extent as not to admit of the ordinary mode of dressing by plasters or cataplasms; an *emollient, desiccative*, or *stimulating liniment* may be used, according to the intention required.—In the latter intention, which will be indicated by an *atonic, sluggish* state of the parts, the liniment of Mr. Kentish, viz. the *linimentum terebinthinæ* of the present *London Pharmacopœia*, is doubtless a very appropriate one—using, as occasion may require, to the sores, after the *eschars* or *sloughs* have separated, or in the act of separating, *desiccatives*, or, in case of fungus rising, *gentle escharotics*; never omitting withal, wherever it can be conveniently applied, especially in the instances of exuberant granulations, *dry lint* and *a moderately tight bandage*.

It is scarcely necessary to observe, that any *cerate*, or *unguent*, may be reduced to the state of a *liniment* by the addition of a due proportion of olive oil.

Since it is essential, according to my opinion, that the parts injured by *burns* or *scalds*, especially immediately, and for a short time after the accident, should be kept as much as possible from the action of *atmospherical air*, and likewise as cool and moist as possible; I have lately adopted the method of continuing the same wrapping linen on, observing to keep this constantly as cool and moist as possible, by frequently dabbing it over with the *saturnine liquor*, by means of a sponge or otherwise—having found the more assiduously this mode is pursued, together with

* This cerate, although an excellent healing desiccative when of a *hardish* consistence, is extremely injurious if of too soft consistence, especially in hot weather.

the

the exclusion of air, the more successful is the process; and moreover, excepting on such parts as may be already denuded of the *cuticle*, I omit the *dressing of cerate*; finding by this mode the inflammation is best prevented, or arrested in its progress, and thereby future vesications obviated.

I consider all *unctuous applications* as extremely injurious to recent *scalds* or *burns*, and particularly so in warm weather, unless constantly covered with a *cold lotion*, according to the manner above mentioned*.

Amongst various other cases of a similar nature, which have occurred to me, treated in the same manner with equal success, I shall briefly mention one which has recently presented itself.

A gentlewoman of a corpulent habit, and strongly inflammatory diathesis, burned both her hands very much in endeavouring to extinguish a fire which happened to the curtains of the bed she was lying on, and from which she escaped with difficulty before the whole was in a blaze. On my arrival I found her immersing her hands in cold water. This I immediately changed for *strong saturnine liquor*, to which I added a very small portion of *camphorated spirit*. Her hands were kept immersed in a liquor of this kind, changing it, as often as it was becoming warm, for fresh. This process was continued from about eleven in the morning until about nine at night, by which time the intolerable heat and pain in the parts were very much diminished, though not entirely subdued. Each hand was then placed separately in a linen bag, containing each a cold cataplasm composed of *bread* crumbled fine, and the same kind of liquor in which they had before been immersed, made very moist, and plentiful in quantity, so as to cover them *thickly*, immersing them occasionally in a bason of the same liquor, to keep them moist and cool.

This plan of poulticing, and moistening occasionally, renewing the cataplasm every night and morning, was continued for four days; by which time they weré in a manner well, a few small places excepted, in which the effect of the fire had not been prevented from penetrating through the *cutis*; and this, I apprehend, might have arisen from want of due attention, during my absence, in not changing the liquor sufficiently often; supposing the fire not to have produced this effect before the application of the preventive means commenced. They were then, for the first time, dressed with the *drying saturnine cerate*, immersing them occasionally, when they were becoming painfully warm, in the cool liquor. By this treatment the effects of the fire were so entirely arrested and obviated, that all was well within the

* The same observation applies likewise to the treatment of all kinds of sores accompanied by intense inflammation.

week from the day of the accident, the small parts before alluded to excepted, and which were almost too inconsiderable to have been mentioned. It is worthy of notice, that during the greater part of the day on which the accident happened, so long as the hands were kept immersed in the cold liquor, they were easy; but if taken out for ever so short a time, the sensation of a most painfully burning heat came on—and likewise, if the liquor in which they were immersed was permitted to become *warm* before changing. The only injury the parts sustained, was an almost entire separation of the *cuticle*—the *cutis* itself remaining perfect, the instances before mentioned excepted. The *vesications*, which were many and very large, I punctured the day after the accident happened, and pressed the serum out; and on the fifth day I removed the greater part of the *detached cuticle*—the *cutis* itself being dry and free from abrasion:—not the slightest inflammation came on.

This patient, residing at a distance from Oxford, returned home well on the sixth day from the accident, with scarcely any other remains of the injury than tenderness, arising from the recently denuded *cutis*. Whereas, I consider my experience warrants me in asserting, that had she been treated in the first instance upon the ordinary stimulating plan, viz. by *camphorated spirits*, or other articles of a similar nature, *ulceration* would have been the consequence, and the cure would probably have taken up more than as many weeks to have accomplished.

I prefer *bread* rubbed or grated small in these cases, for making the cataplasm, to *linseed flour*, because the latter is more apt to *heat*, and moreover does not so readily admit of an equal diffusion of the cooling liquor by immersion.

I forbear entering here into the *secondary* or subsequent mode of treatment necessary, when *eschars* or *ulcerations* ensue from *burns* or *scalds*; my object, in this paper, being to confine myself to the *primary* or *immediate* treatment of such cases.

An attendant upon this gentlewoman had likewise her hands burned at the same time, but in a much less degree. By pursuing the same plan of immersion throughout the day, and the application of similar cataplasms at night, by the next morning they were nearly well.

I have witnessed almost every gradation of *burns*, from the slightest to the most important. The most dreadful instance of these, and life still remaining, was a woman who was so terribly burnt, that the whole surface of her head, body, and her limbs, for the greater part, exhibited an appearance resembling wood, or any other combustible substance, *completely charred*. She lived several hours; during which time her sufferings were in some degree alleviated by the application of cold, damp compresses,

presses, frequently renewed, and the administration of cooling liquors.

To these observations, already too long perhaps, I shall subjoin a circumstance or fact, which perhaps is not irrelevant to the present subject; and which by a kind of inverse analogy may tend to confirm, if further confirmation were wanting, the propriety of the mode of treatment I have recommended in *burns* and *scalds*. By the topical application of *extreme cold* to the skin, as in handling polished metal at a very low temperature, the same effects are produced as in the instance of a *burn* produced by heated iron, &c. viz. *vesication*, &c. In like manner, as the application of *cold* in burns or scalds is a *specific remedy;* so likewise is the application of *heat* in this instance—each used discretionally; viz. by immersion of the part in a liquid of *opposite temperature*, duly adjusted. Moreover, instances of the same kind, in a smaller degree, are very familiar to me, in extreme cold weather, viz. in myself, as well as others. By frequent immersion or washing the hands, at such a season, in cold water, and exposure to the cold air, the tips of one or more of the fingers (these parts from the thinness of the skin there being most susceptible of such an effect, and even without any abrasion of the skin,) become tender, proceeding onto *vesication* and *ulceration*, if this mode be persisted in; whereas, by avoiding the cause, and using warm water, and defending them from the effect of the cold air by wearing a glove, the progress of the injury is checked at the commencement; or, by appropriate dressings or cataplasms, together with immersion in warm water, and protection from cold, which is indispensable, are, in an advanced state, cured. I have several times, for want of attention in time, experienced myself the effects of this kind of injury, as far as to the *vesication* and *separation of the cuticle;* and in other persons, on to a state of *ulceration*. Even in an incipient state of this affection, the application of cold water, or exposure to cold air, is extremely painful, and in an advanced state, intolerably so; whereas, on the other hand, immersion in *warm water* immediately restores, or produces perfect ease. The affection I am speaking of is not to be confounded with that of *chilblain*, from which it differs materially; the effect arising from the sudden communication of intense cold to the skin, or surface of any part, by a quick or powerful conductor of heat, as *polished metal*, or the frequent repetitions of it, in a slighter degree; thus producing effects similar, by an inverse cause, to handling *hot iron*, or subjecting the hands repeatedly to *very hot water* *.

* It will be readily understood, that in this instance the *conductor* operates by *taking away* the heat from the part.

I have

I have not unfrequently traced the mild or superficial species of *paronychia* (*whitlow*) to the frequent application of cold, the end of the finger or thumb having been previously accidentally pricked, or slightly injured by some means or other, and neglected.

With respect to my opinion of the nature of the injury received in *burns* and *scalds*, and *rationale* of the mode of treatment to be adopted in such cases, it is briefly this—that the effect continues to go on, in a certain degree, although the *direct application of the cause* has ceased to act; as is apparent, I think, by the painfully burning heat which remains.

Hence the immediate mode of treatment is evidently pointed out to consist in the application of *cold*, and which I have no doubt, if effectively used, would completely arrest or prevent the progress of such effect, leaving the parts, as far as the *cold* could immediately penetrate, precisely in the same state as at the instant of commencing the process.

The same mode of reasoning will apply equally to the injury produced by an inverse effect to the former, viz. the *direct application of extreme cold* to the surface of the body or limbs, and which, from its similarity in effect to *burns* and *scalds*, has not been unaptly termed *cold-burning** ; the effects in one instance being produced by the sudden *introduction* of *heat* or *caloric*, and the other by the sudden *abstraction* of it, by means of the most powerful *conductors of heat*, allowing for the difference between the *positive* and *negative* mode of action in the two instances.

For obvious reasons, I think, the best mode of applying *cold* or *heat*, in such cases, is by the medium of a *liquid*, and communicated by *immersion*, in all instances which admit of it; and although I have mentioned the degree of 50° of Fahrenheit as the fittest temperature, it will be necessary to attend to the feelings of the patient, and regulate the degree of cold applied, throughout the process, to that temperature which affords most ease.

In the course of my professional experience both in physic and in surgery, I have rarely found plain reason and fact at variance with each other; and I think we shall not err in admitting, in the present instance, that *cold* is the genuine and proper antidote for the effects of heat; and *vice versâ*.

It will however be obvious, that the application of the remedy, in all instances, should be proportionate to the manner in which the injury is received, or communicated: thus, if it be

* *Mercury frozen*, or any polished metal at the same temperature, will quickly destroy the living principle of the skin, and produce *vesication*.

direct

direct and sudden, the mode above recommended should be pursued: if indirect, and in consequence of long exposure to the cause, as is ordinarily the case in the effects produced by *cold;* heat, as in this instance, should be cautiously and gradually applied, with friction, &c.

P. S.—My experience warrants me in asserting, that in all instances of *burns* and *scalds*, which admit of the total exclusion of the external air, as in the mode of *immersion* and *poulticing* before mentioned, the best method is to omit entirely, during that process, any dressing to the part, although the *cutis* be left denuded.

To this mode I attribute the complete success in the last instance, in which case much of the *cuticle* was entirely destroyed or detached by the fire; the *sedative* and *astringent* qualities of the saturnine preparation acting conjointly with the *cold*, in constringing and closing the orifices of the cutaneous exhaling vessels, and thereby preventing effusion, and subsequent suppuration. It will be apparent that the *poulticing* is merely a modification of the same principle as immersion, for the sake of convenience, and may therefore, under particular exigencies or circumstances, be commenced at any time.

To Messrs. Nicholson and Tilloch.

IV. *On the Agency of Electricity in constituting the peculiar Properties of Bodies, and producing Combustion.* *By Mr.* John Webster.

The secondary causes which operate throughout nature are formed with such infinite wisdom and design, by the first great inscrutable cause, that it cannot excite our surprise that finite creatures are unable to fathom or explain these laws, or that there should be various hypotheses concerning them.

Although the advancement that has been made in the chemical department of nature is still limited, yet enough has been done to show the simplicity of her laws and the generality of their operations. When we see principles widely diffused, we conclude that they are intended for great and important purposes in the œconomy of nature, and that general effects are derived from these prevailing causes.

Light, *heat*, and *electricity* are imponderable elements of universal distribution, and appear to be primary instruments in the hands of nature. In discovering her laws, we are more induced to speculate on tangible and sensible masses, than on these universal principles.

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We consider the earth as composed of a certain number of elements, and that the different properties of bodies depend upon the different combinations of these elementary substances. Aggregated masses undergo no change in their properties, without some chemical change amongst the elements that compose them; and as chemical effects are now ascribed to the influence of electricity, the change of property which bodies obtain seems to be more peculiarly derived from a variation in the electrical state of them than from any other source.

The most acrid and offending bodies lose their destructive properties by chemical or electrical combination. If it be the property of acids and alkalis to be corrosive, why do they lose their respective properties by combination, if it be not derived from the electrical union of the ultimate particles that compose them? Even the same elements in the same quantities will receive different properties under different states of electricity. The gas called nitrous oxide, for instance, is, according to Sir H. Davy, composed of thirty-six parts in a hundred oxygen, the remainder nitrogen; and the atmospheric air twenty-one parts oxygen, and the rest nitrogen, saving some inconsiderable portion of carbonic acid gas. Now suppose fifteen parts oxygen be added to the atmospheric air to bring the quantities of these ponderable elements equal; in this case, we are well aware that the properties of the two gases in the lungs are of a very different description, although their elements are the same, and yet the atmospherical air may be converted into the nitrous oxide by electricity.

According to Cavendish's experiment, if we pass electrical shocks through a confined portion of atmospherical air, we produce nitrous gas; but in this experiment a portion of the nitrogen is rejected, and the electrical combination of the oxygen with the remaining nitrogen is in greater proportion than in the atmospherical air: if we present even an oxidable metal to the nitrous gas, it loses a portion of its oxygen, and is converted into nitrous oxide.

The different properties of the two elements, oxygen and nitrogen, in these three different states of combination, seem peculiarly to point out how much the properties of bodies are produced by their different states of electricity.

The same principle is strikingly shown from the effects produced by the vinous and acetous fermentations. Oxygen, hydrogen and carbon compose saccharine matter: this in solution undergoes an electrical process called the vinous fermentation, and alcohol is produced from the same elements as the sugar and water. Another electrical change called the acetous fermentation absorbs a further quantity of oxygen, and wine is converted

verted into vinegar. Thus the properties of the fluid change, as the chemical or electrical combination varies amongst the elementary principles that compose it.

If it be admitted that the character and properties of bodies be derived from electrical influence upon the particles that compose them; the substances themselves are passive, or are merely the vehicles of limited quantities of electricity for which they have a determinate capacity.

Supposing, according to the generally received opinion, that all bodies possess electricity, but in different quantities according to their capacity, *combustion* and other chemical effects might be explained upon a new theory.

The term combustion is here employed to express the evolution of light and heat, rather than the decomposition of a body.

According to Lavoisier, combustion arises out of the combination of an inflammable body with oxygen: hence oxygen has been considered as an essential to combustion.

Inflammable bodies, generally, have great capacity for electricity, and oxygen of all other bodies the least. Whether we suppose the inflammable body and oxygen to be taken away, or the ultimate particles of these bodies combined; still the electricities remain the same, and these very opposite states will rapidly combine to restore an electrical equilibrium. In either case, an emission of light and heat will be produced; that is, either with or without the grosser bodies. So that we have all the effects of combustion in an electrical spark obtained from the conductor of an electrical machine, or from the positive and negative ends of the wires of the electro-chemical apparatus, as well as from the combustion which is produced by the electrical combination of the ultimate particles of bodies in opposite states of electricity.

Oxygen, which is considered as indispensable to combustion, may be withdrawn when electricity is supplied.

In the ordinary experiment of burning charcoal between the positive and negative ends of the electro-chemical apparatus, the degree of light and heat which is evolved is in proportion to the intensity of electrical power employed, and the effect which is produced is greatly superior to any other means of combustion: this seems to arise from the influence of uncombined electricity.

When combustion takes place from the combination of the particles of two bodies, the effect is limited; or the emission of light and heat is dependent upon the electrical capacities of the bodies employed: thus, the different combustible dispositions which different bodies possess arise from their varying capacity for electricity, the intensity being the greatest in those bodies that are in the most opposite states. But the influence of un-

combined electricity in supporting combustion, or producing an evolution of light and heat, is always the most powerful, from the extreme states of the fluid; and equally perfect in effect, whether the quantity employed be great or small: it is on this account that the ordinary electric lights are much more brilliant than those that are obtained by any other means.

The influence of oxygen does not appear to produce any effect where uncombined electricity is employed.

If a combustible body, charcoal for instance, be placed in a vessel of nitrogen or hydrogen gas, on connecting it with the two ends of the electro-chemical apparatus, the light is equally brilliant with that which is obtained in atmospherical air: or, if the charcoal be placed under an exhausted receiver, an equal effect will be produced. The combustion of the charcoal is equally vivid in water or carbonic acid gas; but in the two latter instances it might be objected that the bodies were decomposed, and oxygen supplied from them.

Then briefly to sum up this view: the evolution of light and heat, or the general effects of what is termed combustion, appears to arise out of the rapid union of opposite states of electricity, either independent and uncombined, or effected by means of certain bodies in which it resides; and that the combination of the ultimate particles of sensible masses can only take place through the influence of those portions of electricity for which the bodies have a specific capacity; and that the properties of these bodies arise out of their electrical states: thus generally tending to support those established principles of Sir H. Davy, that all chemical combination is dependent upon electrical union.

If these observations should be found worthy of a place in your very useful publication, I shall take another opportunity of extending this subject a little further, and of tracing some of the analogies of light and heat with electricity, and the influence of these universal agents in the œconomy of nature.

I am, sir,

Your obedient servant,

John Webster.

To Messrs. Nicholson and Tilloch.

V. *On Electricity.* *By* George John Singer, *Esq.*

Some time since, I ventured to rectify some erroneous statements of Mr. Ez. Walker's on the subject of electricity; regarding them as the mistakes of a novice in that science, which it might be useful to correct for the advantage of other young electricians.

Had

Had I then imagined that the "*opinions and suppositions*" with which they were accompanied were intended to form the basis of a new system of "chemical philosophy," or to be followed by the luminous views of *electrical combustion*, and *oxygen and hydrogen electricity*, which have been developed (at page 267 of the forty-second volume of the Philosophical Magazine), I should certainly have avoided any notice of those "statements," and most probably might not then have incurred the anger Mr. Walker has manifested against me in his note published in the last volume of this Magazine *.

It would be unnecessary to reply to that note, but that it contains an assertion of "the thing which is not." I am represented as having asserted that Mr. Walker is in *error*, without adducing *one fact* to prove it. To refute this misrepresentation, it is only necessary to repeat here my former paragraph, from which Mr. W. has made some mutilated extracts. It was printed as follows:

"With respect to the *permanence* of the effects produced by electrical influence, Mr. W. has fallen into error by confounding them with *communicated* electricity. If, after bringing an electrified body near an insulated conductor, on withdrawing it the insulated conductor remains permanently electrified, it must have *lost* or *received* electricity; and in either case it is electrified by *communication*, and *not* by *position*, whether its loss or gain be the consequence of the contact of some conducting body, or the imperfection of its own insulation during the disturbance of its natural electricity; and *one of these causes must operate* to produce *permanent electricity* in such an experiment: for *neither an insulated rod*, nor a *gold-leaf electrometer, if properly constructed*, will be *permanently* electrified by approximation to an electrified body; unless they *communicate* by *imperfect* insulation, or *pointed terminations, with surrounding unelectrified substances during such approximation*. These are facts, which the constant repetition of such experiments professionally enables me to state with confidence; and they are indeed such as amongst electricians are generally admitted: but perhaps Mr. Walker has yet to learn, that a conducting body supported by dry glass, and surrounded by dry air, may be still very far from perfectly insulated †."

The statement in the preceding paragraph may be verified in a few minutes by any one sufficiently acquainted with the practice of electricity to make experiments with due accuracy; and I believe it not only adduces a *fact* in proof of Mr. Walker's *error*, but offers the requisite information to show him its cause.

* Vol. xlii. p. 485.

† Phil. Mag. vol. xlii. p. 264.

Independent

Independent of this circumstance, it is amusing to find an individual so confident of the infallibility of his own observations, as to consider them sufficient to subvert the experience of Canton, Franklin, Wilke, Æpinus, Cavallo, Stanhope, and Robison; although the observations of the most experienced electricians of the present time are opposed to the inference he has drawn.

London, Jan. 10, 1814. G. J. SINGER.

To Messrs. Nicholson and Tilloch.

VI. *New Outlines of Chemical Philosophy.* *By* EZ. WALKER, *Esq. of Lynn, Norfolk.*

[Continued from p. 371, vol. xlii.]

MUCH confusion in chemical philosophy seems to arise from want of precision in chemical language. Oxygen and hydrogen are terms that are used in a very vague manner, no distinction being made between oxygen and oxygen gas, nor between hydrogen and hydrogen gas. Positive and negative are words that have no definite meanings in chemical science; and yet we have positive and negative electricity, positive and negative galvanism; and even the laws of chemical affinity are now explained by the terms positive and negative. We have also fixed fire, phlogiston, and other words that express things rather more imaginary than real. All this confusion might be avoided by adopting two new terms to express the two elements, which produce effects that are real objects of our senses, and by which those elements may be known in all their various conbinations with matter.

"Mr. Davy," now Sir Humphry Davy, "exhibited the powers of the Voltaic instrument by brilliant experiments; metals were fused upon the surface of water and of oil of turpentine, and burnt in contact with them. He stated that the maximum of heat was at the positive electrical surface; and he exhibited an experiment in which, though the most brilliant light was at the negative surface, yet the ignition was infinitely greater at the positive*."

From these properties of the two elements above mentioned, that element which produces the maximum of heat, at the positive electrical surface, may be called the generator of heat, or *thermogen;* and that element which appears at the negative surface, and exhibits the most brilliant light, may be called the generator of light, or *photogen.* The choice of these terms seems to be sanctioned by the words *thermometer* and *photometer* now in common use. By the term *photogen* is to be understood the imponderable element of all combustibles, whether it

* Phil. Mag. vol. xxxix. p. 137.

exists

exists combined or uncombined with matter; by *thermogen* is to be understood the imponderable element of all supporters of combustion, whether it exists combined with matter, or in a free and uncombined state.

Thermogen and Photogen.

When a Leyden jar is receiving a charge from an electric machine, one side receives *thermogen* from the atmosphere, and the other side receives *photogen* from the earth. When a communication is made between the inside and the outside of the jar, by some conductor of these elements, the element from the inside attracting the element from the outside, a spark is produced, consisting of a double current exhibiting both light and heat, and the equilibrium which was destroyed by the action of the machine is restored. But it cannot be supposed that these elements are annihilated by their union: such a supposition would be absurd: they only enter into new combinations, and return again into those receptacles from whence they were withdrawn. For while the element of one side of the jar goes to the earth, the element of the other side is communicated to the air.

When two portions of the same element are nearly equal they repel each other, but when they are very unequal they attract each other. This is known from an infinite number of experiments: consequently, the immense attraction of the photogen in the earth attracts any small portion of the same element, which may be disengaged from its base, with an infinite force; and the thermogen in the atmosphere attracts every small portion of the same element the instant that it is disengaged from matter.

But why does not the thermogen in the atmosphere unite with the photogen of the earth, as they have a very strong attraction for each other? This question will be easily solved as soon as it is understood that they are kept asunder by their bases. Oxygen and hydrogen gases mixed together in a proper vessel, would remain for ages without producing any effect upon one another; but let their elements be brought into contact, either by pressure or increase of temperature, and combustion will instantly take place.

If the attraction between these elements of combustion and their bases were not proportioned to a mathematical exactness, the whole œconomy of our globe would be deranged. For, if this attraction were greater than it is at present, we should have perpetual frost; and if this attraction were much less than it is, all combustibles would be reduced to ashes in a moment: but, in the present state of things, it is impossible that either of these events should happen.

It is not to be understood that photogen is light, or that

thermogen is heat; they are the generators of light and heat, light and heat being the effects which they produce on matter. The thermogen on the inside of a charged jar cannot be felt, nor does it produce any effect upon the most delicate thermometer, nor is any light perceptible on the outside of the jar. But let the jar be discharged through a piece of iron wire, and both light and heat are produced. When the metal becomes red hot by this means, it loses its photogen, attracts oxygen gas from the air, and becomes oxidized. If this oxide be mixed with some matter containing photogen, and heat applied, oxygen gas will be reproduced, and the metal will receive photogen, and again become malleable.

The application of these elements, to elucidate some of the most interesting phænomena in nature, will be more fully treated of hereafter.

Composition of Water.

Some philosophers maintain that water is a simple body, and the only ponderable basis of oxygen and hydrogen gases, and other aërial fluids; but others suppose that water is a compound of oxygen and hydrogen, and this opinion is now generally adopted by writers on chemistry. But all the experiments made to separate the component parts of water, tend to prove that it is a body simple and undecompounded. What has led men to conclude that water is a compound body, seems to have arisen from their supposing that the electric spark is a simple fluid; but as it is now well known that it consists of two elements possessing different properties, both *chemical* and *mechanical**, the passing of these elements through water is no proof of its being a compound substance, notwithstanding oxygen and hydrogen gases are produced by this means.

When thermogen and photogen pass through water in contrary directions, thermogen and water form oxygen gas; photogen and water form hydrogen gas. These gases being mixed together in a close glass vessel, and their temperature increased to a certain degree, combustion takes place, light and heat escape through the glass, and the product is pure water.

That thermogen and photogen are capable of producing combustion, before they entered the water, is unquestionable, nor can it be supposed that they are annihilated by passing through it; for they appear again in the two gases, producing the same phænomena of light and heat as before. Hence it appears that the two elements which compose the electric spark are the imponderable elements of the two gases, and that water is their common base. Oxygen and hydrogen gases thrown upon fire pro-

* Phil. Mag. vol. xlii. p. 161.

duce

duce the most intense heat; but water being thrown upon fire extinguishes it: water, therefore, is neither a combustible nor a supporter of combustion, and consequently it contains neither oxygen nor hydrogen.

Another experiment, which has been made to prove that water is a compound of oxygen and hydrogen, consists of passing steam through a red hot iron tube containing turnings of iron, by which means hydrogen gas is produced, and the iron turnings become oxidized; whence it is concluded, that water is a compound of oxygen and hydrogen. But let the experiment be tried with a red hot glass tube containing pieces of broken glass, and no hydrogen gas will appear: hence it is evident, that one of the component parts of the hydrogen gas, in the former experiment, came from the metal.

Now when the turnings of iron become red hot, the photogen they contain and water produce hydrogen gas: but in this process thermogen must be present, for no combustion nor increase of temperature can be *generated* unless thermogen and photogen be united. The most condensed rays of the sun do not excite combustion in vacuo, nor in any gas deprived of oxygen, even when the most inflammable substance is employed*. Consequently, the thermogen and water produce oxygen gas, which the iron, after being deprived of its photogen, attracts, and becomes an oxide of that metal.

This experiment may be illustrated by the burning of iron wire in oxygen gas. In this beautiful exhibition the most brilliant light and the most intense heat are generated, by the union of the photogen of the metal and the thermogen of the oxygen gas. The metal, having lost its photogen, attracts oxygen gas, and becomes an oxide of iron intensely heated.

The photogen or generator of light in the metal cannot be hydrogen, for "no combinations of iron with hydrogen or azote are known †." If this assertion be correct, we have combustion without hydrogen.

Sir H. Davy observes, that "the nature of water may be shown synthetically as well as analytically."

"When ten grains of the metal called potassium are added to about two grains of water in a glass tube, there is a violent action, much hydrogen is disengaged, and by heating the results the operation is completed. The same effect is produced upon the potassium, as would be produced by heating it strongly in contact with a small quantity of oxygen; it becomes united to oxygen, and its increase of weight is in proportion to the weight of the hydrogen as 15 to 2 ‡."

* See Phil. Mag vol. xlii. p. 370.

† Davy's Elements, p. 388.

‡ Davy's Elements, p. 247.

But

But the composition of water *cannot* be shown by a *compound test.* "Potassium inflames when gently heated, aud throws off fumes which are alkaline *." Consequently potassium is a compound of an inflammable element and a metal, and therefore it may be used as a test to prove that water is a *simple body*, and not a compound. For the inflammable element and water will form hydrogen gas, and the metal when strongly heated will attract oxygen gas in the same manner as other metals.

"It may be remarked, that oxygen is *mild* when in the proportion of 22 per cent. in atmospheric air, and highly *corrosive* in the proportion of 70 per cent. in nitric acid; or even of that of 40 per cent. in sulphuric acid. How is it, then, that it is found in the ratio of 85 per cent. in water, and that this compound, compared with the others, should be perfectly mild and innocent?" The answer given to this question is, "*because its oxygen is so forcibly retained by the hydrogen* †."

Now, as the theory of the compound nature of water does not rest upon common experience, nor upon any known truth, but upon a bare assertion without any proof whatever; therefore, it seems to be nothing more than a fanciful conjecture.

Lynn, Jan. 7, 1814. EZ. WALKER.

To Messrs. Nicholson and Tilloch.

[To be continued.]

VII. *Observations on a Fiery Meteor.* *By* T. FORSTER, *Esq.*

I COMMUNICATE to you the following account of a fiery meteor, which was seen by a friend of mine on Monday evening, the 8th of November last, about eight o'clock. He was walking between Woodford and Hackney; the weather being fine, and the moon shining bright; and was suddenly surrounded by a light blueish flame: it lasted two or three seconds, so as to give him time to turn himself round and view it on all sides. He felt at the time a warmth like that of a very hot damp day. When it subsided, he perceived the moon much dimmer, on account of the bright light which had just affected the organs of vision. These are all the particulars I have of it, and I communicate them just as they were given me. In hopes that some other persons more distant, who may have seen this curious phænomenon, which is certainly of the *ignis fatuus* kind, may give further particulars, I send this for insertion in your Magazine.

Yours, &c.

Clapton, Jan. 10, 1814. THOMAS FORSTER.

P. S. There were many meteors called falling stars on the evening of the aforesaid 8th of November.

To Messrs. Nicholson and Tilloch.

* Davy's Elements, p. 322. † Parkes's Chem. Cat. p. 252.

VIII. *Notes and Observations on the remaining part of the Sixth and part of the Seventh Chapters of Mr.* ROBERT BAKEWELL'S *"Introduction to Geology;"—embracing incidentally, several new Points of Geological Investigation and Theory. By Mr.* JOHN FAREY *Sen., Mineral Surveyor.*

[Continued from vol. xlii. p. 367.]

Notes, &c.

P. 143, l. 5, Loose stones†.—† Rubble, or angular broken stones, are never found *in* the Coal-measures, though such often are *on*, near the surface, in Coal-fields, in common with almost every other part of the stony strata.

144, l. 1, narrower or wider*.—* Thicker or thinner.

l. 10, the roof†.—† Rep. i. 345.

l. 25, called *tow*‡.—‡ Tawe, Duns, &c. Rep. i. 348.

147, l. 6, *raised* on that side*.—* But the Coal and other strata may, on the contrary, be *sunk* on that side, if the *let-down* is greater than the depth of the pit *gg*: a case which not unfrequently happens, and such is evidently the source, of some of those grand mistakes, as to the identity of Coal-seams, with which I have ventured to accuse even the most experienced Coal-masters and agents, Rep. i. 166 and 167, &c. See my 2d Letter, p. 110.

l. 22, shattered where they come in contact†.—† See my Note on p. 208.

148, l. 3, generally serve to identify it*.—* This defective rule, is often very improperly relied on, in speaking of the identity of Coal-seams (as observed in my 2d Letter, p. 111), when far better evidence on this important point, might easily be had, by comparing the entire sinkings of the Pits, and tracing the bassets of the several strata on the surface, between the places.

l. 9, 300 yards†.—† Clandown Coal Pits, SE of Paulton in Somersetshire, are reported to me, to be 400 yards deep!

l. 12, 30 feet‡.—‡ Rep. i. 176.

l. 21, 13 feet thick**.—** Rep. i. 196.

149, l. 9, Bind or Clunch*.—* White stone, Rep. i. 176, and Mont. Mag. v. xxi.

152, l. 25, one-third of England*.—* Which the Lias strata and those above it occupy, Rep. i. 116.

153, l. 18, founderies*.—* Furnaces, Rep. i. 395.

P. 154,

P. 154, l. 11, unfit to make iron *.—* The second unsuccessful attempt with the Earl of Moira's Iron Furnace, on Ashby Wolds (Rep. i. 401), is here I suppose alluded to:—to me it seemed, when there on my Derbyshire Survey, that *the quantity of iron-stone* that could be readily got, was quite inadequate for the supply of the furnace erected. Earl Stanhope's furnace at Dale Abbey (Rep. i. 397) is said to have failed, owing to *the quality of the Coals* in its vicinity, although the ironstone was in plenty, and of good quality.

Noblemen and gentlemen will in time find out, that they had much better take the advice of experienced professional Men, in *the Letting* of their Minerals on proper terms, than attempt *the working* of them on their own account; or, than take the first offer of any rash or inexperienced adventurers, who may apply.

157, l. 21 and 22, an alluvial production *.—* This is certainly the case with a large proportion of our *Wood Coal*, near the surface, as that of Bovey (p. 158) mentioned in a Note in my first Letter, p. 57. But it seems no less certain, that some irregular Coal-seams, having the characters of Wood Coal, are lodged *between regular strata*, that in particular, above the Doggers and alum Shale near Whitby in Yorks. mentioned page 267; which was working at Napehow in 1811, and had formerly been wrought at Newton-house, Rudscar, &c., P. M. xxxix. p. 101. The very irregular strata of wood Coal, above the Pipe Clay, in the Isle (as it is improperly called) of Purbeck, that are I believe alluded to P. M. xlii. p. 396, I have not seen; but from the description by my oldest Son and others, who have seem them, I conclude them to belong to the alluvium.

161, l. 22, after each inundation*.—* If the plants which gave rise to Coal, were all *subaqueous*, or adapted to the bottom of a deep and quiet Ocean, instead of dry land (as I suggested in the articles *Coal* and *Colliery* in Dr. Rees's Cyclopædia), to which I know no opposing facts, but many coroborative ones (Mont. Mag. xxxiii. p. 514 and 515), all the difficulties, of Mr. B's almost infinitely repeated, alternations of dry Land and Water, would be avoided.

Mr. B. has no where noticed the important Geological fact, which I have generalised, I think (and was also the first writer who mentioned it, I believe) of the *floors* of Coal-seams, being all of one nature, viz. infusible

[P.161]fusible *Clay*, Rep. i. 179§; and also have shown, that the extinct *Coaly vegetables*, in this respect, differ essentially from our *Bog Plants*, or such as have formed Peat, which seem to require a *sandy* soil for their growth, Rep. i. 308 and 312.

Besides the mere thin hollow pipes, or thin flat leaves, which distinguish the plants of the regular Coal strata, the British Strata throughout their whole series, almost, present in places, numerous detached specimens of the solid or ligneous parts of vegetables, closely resembling *Wood*, which sometimes, though rarely, are found with the bark on them, (but never with Roots, any more than the Coal plants,) and more rarely with arms or main branches, but without any of the minuter branches or leaves: by far the greater number of specimens of this fossil Wood, whether petrified, carbonated, or rotten, are found in splinters or billets, as if, first forcibly rent from large trees, and afterwards, for a long time floated in water, where they have become worm-eaten, and much worn, in many instances.

These circumstances attending fossil Woods, have greater difficulties attending them, than almost any of the Geological phænomena that I am acquainted with; —when and where did they grow?, how were they so generally rent? and so universally, and through such long periods of time, diffused in the original Ocean? &c. I have been able to perceive no reasons, why the bottom of the primitive Waters may not have produced *ligneous* as well as less solid vegetables, where neither of such might require *Roots* (for such I believe are never found) to support them, owing to the quiet state of the waters, or to nourish them, any more than the corallines of our days.

162, l. 7, may have consolidated *. — * Mr. B. has scarcely taken any notice in his work, of the *kind of crystallization*, by which seams or strata of Coal, are almost invariably found split (or capable of being so), into rhomboidal blocks, by nearly vertical joints; the length-way joints being very generally called *slines*, and the oblique and shorter end-joints, called *cutters*, (see Note on page 69); nor has he noticed, a curious

§ In addition to eight or nine different Names, which I had found established, in different districts of Great Britain, for the infusible Clay, thus forming the *floors* of their several Coal-seams, I lately met with the term *Warrant*, in the Isle of Anglesea, as the general name for the substance and stratum, immediately below each of their Coals.

fact,

[P.162] fact, mentioned by me, Rep. i. 181 and 343, that theses lines very generally, if not invariably?, range about ESE and WNW, through extensive districts, where the dips vary much, both locally and generally!

l. 14, the complete consolidation †. — † See my Note on page 19.

166, l. 22 and 23, nearly horizontal *.—* In a north and south direction, but having a very perceptible dip to the Eastward, Rep. i. 156, 168.

167, l. 1, singularly contorted *.—* Mr. B. seems here to allude to the Wild-Park, Breedon and Clouds-hill, magnesian and contorted Limestone Rocks, as being identical with the yellow Lime Rock, which *overlies* the Notts, Derby, York and Durham Coal-field (see my Note on p. 176); notwithstanding, that they are herein, at pages 284, 275, &c., represented to be identical with, and a continuation of, the Peak Limestones, and also with the shale Limestone near Ashburne, page 286, which last, as clearly *underlie* this vast series of Coal strata, as mentioned in my second Letter, p. 106.

l. 12, quantity of common lime †.—† Rep. ii. 107, 409 and 412.

l. 23, edifices in the metropolis ‡.—‡ The fine Oalite stone, of the vicinity of *Bath*, is now coming into use in London, brought by the Kennet and Avon Canal; the stone using in restoring Henry VII's Chapel at Westminster, is from thence; and in an adjoining Field, my friend Mr. *Wm. Smith* has extensive quarries, and a rail-way thence to the Canal, where he has a Saw-mill at work, preparing it for all the different purposes of the London Mason.

168, l. 9, as has been traced *.—* See the articles *Coal* and *Colliery*, in Dr. Rees's Cyclopædia, and Rep. i. 109.

l. 12 and 13, occur in the South †.—† Phil. Mag. xxxix. p. 94.

l. 23, south of Europe ‡.—‡ The Rev. J. Townsend, who was so well acquainted with our *Chalk* strata, before he travelled in Spain, and on his return, was not likely to mistake Mr. B's ("imaginary") earthy lime-stone, for chalk (see p. 185), nor is it probable, that the Island of *Crete* or Candia, and others in the south of Europe, are without Chalk, as Mr. B. has here insinuated; perhaps this doubt was expressed, in order to invalidate the suggestion alluded to at the conclusion of my Note on page 138.

P. 169,

P. 169, l. 7, upper or soft Chalk*.—* P. M. xxxv. p. 130, and Rep. i. 111.

l. 20, called chert †.—† Rep. i. 272.

171, l. 23, lime and flint are changed *.—* Rep. i. 272, and 415: in Notes on my Paper on the Ashover Denudation, mentioned in a Note in my first Letter (p. 55), several instances are mentioned, and the circumstances stated, under which such or similar changes appear to me to have happened, to different substances, when lodged in the alluvium: to the Slate Rubble on Whittle Hill in Charnwood Forest, for instance, see my Note on Mr. B. p. 291.

172, l. 13, layers of flint *.—* P. M. xxxv. p. 130, and Rep. i. 112.

l. 19 and 20, building-stones †.—† Totternhoe, Reche, and Ryegate stone, &c., Rep. i. 112.

l. 24 and 25, last or uppermost ‡.—‡ That Chalk is not the uppermost regular stratum in England, is now very evident, in the northern part of the Isle of Wight, Mr. B. p. 177 and 335, P. M. xli. p. 224, and 461, vol. xxxv. p. 132, and vol. xlii. p. 395.

l. 25, alluvial ground **.—** The doubts formerly of my friend Mr. Benjamin Bevan, and others, as to the London Clays and Sands being regular strata, P. M. xxxv. p. 137 §, should I think be entirely removed, by the subsequent excavations in Highgate Hill and under Hyde Park, &c. My worthy friend Mr. William Atkinson, Architect, of Bentinck Street, has also successfully investigated the nature of the Chert Nodules, which principally gave rise to the doubts alluded to, by collecting great numbers from the Gravel Pits and Roads near London, and causing them to be sawn through and polished:—they almost invariably prove

§ At page 132 of the volume here referred to, I stated six different suggestions, in the form of queries, regarding the identity of the London and the Paris strata; the fourth of these, on which there is a note at the bottom of the page, relates to a modified or altered nature of these strata (such as is mentioned of Red Marl and of Limestone Shale, Rep. i. 148, and 228; of Gritstones and of Coals, in my Notes on Mr. B's pages 44, 125, &c. &c.); and whereon, an Officer in one of the Societies to whom I recommended the consideration of this question, has lately mentioned, the probability, that the blue Clay of London and the *calcaire grossier* or coarse Limestone of Paris are parts of the same deposit or stratum, "and that from local circumstances the clay has prevailed in the one, and the calcareous matter in the other," P. M. xlii. p. 398.

At the bottom of page 136, vol. xxxv. I suggested, that the *Selenite* of the London Clay (as in the Croydon Canal on Plow-Garlic Hill SSW of Deptford), may answer to the *Gypsum* of the neighbourhood of Paris.

of

[P.172] of a concentric or nodular formation, and contradict the supposition, of their being *rounded* masses of flint, chert, or any other stone: a small shell, proves the nucleus of one of the Nodules: in great numbers, the figures or stripes, curved, fortification-shaped, &c. are very beautiful, and the colours quite equal to most of those which are washed out of the alluvial red Clay on the shore near Whitby in York NR. and are collected from the beach with such avidity, by the Curious, many of whom are not aware, that they might find such at their own doors, near London. Mr. Sowerby's collection has long been rich, I believe, in these small nodular masses, of the London Clay strata.

173, l. 3, extensive Lakes *.—* See my Notes on p. 60, 138 and 182, &c.

l. 17, others foliated †.—† The specimens of Paris Gypsum, containing Bones, which I have seen, corresponded in their characters with none of the above, being earthy and slightly granular; intermediary in structure between Chalk and soft Oalite Limestone.

174, l. 26, well-educated geognost *.—* See page 176 Note, 353 Note, and Rep. i. xlvi.

175, l. 15, *marle* and gravel containing *.—* The organic Remains here mentioned, all occur in *Alluvial* Clay, *on* the Marle or Gypsum and not *in* the Red Marle, here or elsewhere, see Rep. i. 136 and 149, Edinburgh Encyclopædia iii. 393, and my second Letter, p. 105.

176, l. 3, The Geological situation *.—* On first reading the paragraph which thus commences, and Mr. B's Note thereon, below, I could not help thinking, that he was attempting a *hoax*, on the "well educated geognosts," and I was unwilling to suppose, that he was himself serious, in adding to the extreme confusion on this head, with which he charges "the disciples of Werner" in his note on page 174; the point is however of too much importance, as to the real succession of the strata in England, to be thus dismissed, and I will quote Mr. B's passage, with such references or explanations in [ ] as appear to me necessary, viz.

"The Geological situation of the Derbyshire gypsum may be represented, as situated in the upper *secondary* strata, separated [below] from the mountain lime [i. e. *transition* L. p. 92, 93, Map p. 255 &c.], by intervening coal districts on one side, and [above] from the stratified magnesian lime [which is an upper *secondary*

[P.176] *secondary* rock, p. 166] by *sandstone* on the other side."

Now it is evident, from page 166, although the mention of Derbyshire is omitted in describing the magnesian range, from Nottinghamshire to Northumberland, that the *lower* yellow Lime Rock, is here intended, and that the *sandstone* mentioned above, must mean the stratum next under the lower yellow Lime, (often appearing as a *Sand*, but sometimes also as a thick Sandstone Rock, see Rep. ii. p. 410, Note); because, otherwise, either Coal strata would occur on both sides of [above and below] his Gypsum if placed lower, or if Mr. B. referred to the *upper* yellow lime, and made his Gypsum belong to the intervening Red Marl, which I have lately traced between these rocks (P. M. xxxix. p. 104), then "earthy limestone" would occur [below] on one side of it instead of Coal strata; and thus therefore it should seem, that Mr. B. would persuade "the disciples of Werner," to place the Chellaston Gypsum, and its incumbent Red Marl, next above the Derbyshire Coal-measures, and under the yellow Lime Rock, which covers them!—But has any Collier, *sinking through the Limestone to work the Coals* below, (as several do) ever seen an ounce of Gypsum?, or has such anywhere been found at its basset?, or even Red Marl, which might contain it?.

What the sandstone (or Gravel Rock, Rep. i. 132) west of Nottingham, has to do with the Gypsum of Chellaston Hill, in Mr. B's Note, p. 176, might puzzle a much better educated Geognost, than I pretend to be.

l. 12 and 13, in the gravel and marl †.—† Marl has here been inserted by mistake, see my 2nd Letter (p. 105) and note on page 175.

l. 15, a distinct formation ‡.—‡ The mistake on which this supposition is founded, is pointed out as above, see Rep. i. 147.

177, l. 3, over chalk *.—* When the principles and practice of Mr. *William Smith* and others, in exploring and *tracing the strata*, shall be extended to the Continent, which hitherto has very imperfectly been the case, as it would appear, from the large Map of the strata around Paris which was lately published, the calcareous strata here alluded to, and doubtless many others above them in the series, will, I think, be very extensively traced, in connection, as well as seen in iso-

 lated

[P.177] lated *hummocks*, beyond their regular edges; see my note on page 336.

l. 8, upper part of the Clay †.—† See my note on page 16.

l. 18, fresh-water shells‡.—‡ See my note on page 60.

179, l. 6, *seas* of salt and fresh water *.—* The Parisian Geologists speak not of Seas, but of *Lakes* of *fresh* water, I believe, and which are sufficiently improbable, see P. M. xxxv. p. 134, and my note on page 182.

181, l. 21, in their present situations *.—* It has I hope been unintentional, that the true situation of the remains of *large Quadrupeds* near Paris, has, both here and at page 336 (No. 13), been omitted; although they are so expressly described, by M. Cuvier and his associates (see P. M. xxxv. p. 58), to be found *in the most recent alluvium, and not in the stratified Clay* beneath, as has in so many places here, been mistakingly represented, with respect to similar Remains, found near London: see my Note on page 16.

Sir Joseph Banks has very frequently mentioned to his friends, this idea (ascribed by Mr. B. to La Metherie), as to the Bones of large Quadrupeds, and Shells, supposed to be of *fresh-water* origin, found with them, having been drifted *from the Land*, by inundations; and he has referred to *the situations* of the two most noted depositaries of them near London; just below the junctions of the Brent and of the Roding Valleys, with the larger one of the Thames, near to Brentford and to Barking, as presenting probable reasons for their accumulation in these particular spots, by eddies in the meeting currents. Soon after the period of these depositions, it appears to me, that a comparatively quiescent state of the inundation recurred, and the alluvial deposit of *loam* or Brick-earth, so plentifully found *upon* the London *Gravel*, took place, and covered, and has preserved these Bones, &c.

182, l. 4, of the great rivers *.—* But where do great Rivers, or Lakes supplied by them, deposit *regular strata?* or any thing which can be mistaken for such strata, when examined with scientific attention?

[To be continued.]

IX. *On*

IX. *On the Formation of Fat in the Intestines of living Animals. By Sir* Everard Home, *Bart. Presented by the Society for promoting the Knowledge of Animal Chemistry**.

The investigation of the digestive organs of different animals, in which I have been engaged for many years, has led me imperceptibly into an inquiry respecting the particular uses of the lower portion of the intestines in birds and quadrupeds.

The first thing that attracted my notice more particularly to this subject, was finding that, in all animals whose stomachs are made up of a great variety of parts for the purpose of œconomizing the food, the colon has a greater extent of surface, and the course of the canal is so disposed that its contents must be a long time in their passage through it. This circumstance led me to believe that the food, after the chyle is formed and separated from it, undergoes in the lower intestines some changes, by which a secondary kind of nourishment is extracted from it.

This opinion was much strengthened, by finding that the colon of the casuary from Java is only one foot long, and each of the cæca which are appendages to it, only six inches long, and a quarter of an inch in diameter; while the African ostrich has the colon forty-five feet, and each of the cæca two feet nine inches in length, and at the widest part three inches in diameter: besides which, both the colon and cæca have very broad valvulæ conniventes not met with in the casuary from Java. This wonderful difference, for it is more than fifty to one, can only be explained by the luxuriancy of Java being so great, that this bird might destroy its health by over-feeding, had no guard been furnished by nature.

This guard is, the food passing through the intestines with so much facility, and in so short a time, that, however much the bird may eat, only the necessary quantity of nourishment is carried into the constitution; but in the African ostrich, the food is retained in the extensive colon till every thing nutritious is extracted. In all ruminating animals, the colon is of great length, is fixed in its course, which is very intricate, and varies in every different genus; so that we cannot doubt of some particular process being carried on in it.

The process which the contents of the colon undergo, is quite distinct from any thing carried on in the other intestines, since they entirely change their appearance and smell; and there is commonly a valve to prevent any part of them, even the gases evolved, from being carried up into the small intestines.

The peculiar smell of the fæces, which borders so closely on that

* From the Philosophical Transactions for 1813, part ii.

that from putrefaction, although by no means the same, led me to compare them with the animal matter buried in the earth, which is converted into adipocere: in both cases the substance is in the incipient state of putrefaction, but that process never completely takes place; it is excluded from the external air, is either under water, or within the reach of imbibing moisture; and there is no substance whatever, the chyle excepted, which can better supply the waste produced by the actions of growth and muscular exertion, than animal fat.

The more I canvassed this new opinion, the greater number of circumstances in favour of it occurred to me; one of the strongest of which is, that there is no other mode I am acquainted with, by which animal fat can be formed. To this may be added the curious circumstance of the sleeping animals, which lay in so large a supply of it, in a short time, to serve for their winter's consumption, having a formation of the intestines almost peculiar to themselves, in which there is no valve to distinguish the colon, and no fixed course for that intestine; so that the contents pass along with more facility, and remain a shorter time in the canal, the food being sufficiently plentiful during the summer to compensate for this want of œconomy, by which the lower intestines receive more abundant supplies for the production of fat. These intestines remain empty during the sleeping season, so that no fat can be formed in that period.—With this very important information, thus procured, in support of my opinion, I have been led to prosecute this inquiry with increased ardour, and shall now bring forward the facts I have been able to ascertain in confirmation of my hypothesis. These I shall detail in the order in which they were acquired, thinking it better to lay before the Society the regular process of the investigation, than to grasp at once at the conclusions which in the end of it I have felt myself authorised to draw.

I shall therefore begin by stating the circumstances under which adipocere is formed from animal matter, most nearly resembling those in which the contents of the lower intestines in living animals are placed; and this I shall do from facts, entirely within my own knowledge, the specimens of the adipocere being now in my possession; and afterwards go on by bringing forward proofs that a substance similar to it is formed in the colon.

Mary Howard, aged forty-four, died on the 12th of May 1790, and was buried in a grave ten feet deep at the east end of Shoreditch churchyard, ten feet to the east of the great common sewer, which runs north and south, and has always a current of water in it, the usual level of which is eight feet below the surface of the ground, and two feet above the level of the coffins in the graves. In August 1811 the body was taken up, with some others

others buried near it, for the purpose of building a vault, and the flesh in all of them was completely converted into adipocere or spermaceti. In Stowe's History of London, this part of Shoreditch is stated to be a morass, and since that time the ground has been raised eight feet. The clerk and the grave-digger observe, that at the full and new moon the water in the sewer rises two feet, and that at those times there is water found in the graves, which at other times are dry.

The current of water, which passes through the colon, while the loculated lateral parts are full of solid matter, places the solid contents in somewhat similar circumstances to dead bodies in the banks of a common sewer.

The circumstance of ambergris, which contains sixty per cent. of fat, being found, in immense quantities, in the lower intestines of the spermaceti whales, and never higher up than seven feet from the anus, is an undeniable proof of fat being formed in the intestines; and, as the ambergris is only met with in whales out of health, it is most probably collected there from the absorbents under the influence of disease, not acting so as to take it into the constitution.

Ambergris is found in lumps from fourteen to more than one hundred pounds each; it is not to be distinguished in its appearance from the fæces, but when exposed to the air it grows hard; a lump has been found in the sea weighing one hundred and eighty-two pounds *.

In the human colon, solid masses of fat are sometimes met with in a diseased state of that canal, and are called *scybala;* these are in all respects similar to ambergris.

Concretions of olive oil and mucus found in the human intestines must be formed in the same way. A case of this kind was communicated to me by our associate Dr. Babington in the following letter:

"My dear sir, 17, Aldermanbury, Feb. 2, 1813.

"The following are the circumstances relating to the change produced upon olive oil, by passing through the stomach and intestines of the elderly person whose case I mentioned to you at the last meeting of our Animal Chemistry Society. The lady in question had for several years past suffered from severe affections of the stomach, which, from the attendant symptoms, were considered as occasioned by the irritation of biliary concretions. Many remedies having been resorted to without affording her other than temporary benefit, she was advised to try the effects of olive oil, taken to the quantity of two or three ounces at a time, and to be repeated as circumstances might require. From

* Vide Phil Trans. 1783.

this she experienced almost immediate relief; and, on the subsequent examination of what passed from the bowels, globular concretions were uniformly observed, which by the persons about her were considered as the gall-stones which had previously been productive of so much distress. This lady having occasion some months since to visit her friends in town, and a doubt having been suggested by one of her medical attendants in the country, as to the nature of the concretions in question, I was desirous, from the account that I had received, to have an opportunity of determining the point for myself; and therefore requested, that if the pain should recur, and she should be under the necessity of repeating her medicine, that the concretions, which had been said always to pass from the bowels in consequence of her so doing, might be reserved for my inspection. In a few days I was summoned to make my proposed visit; and, upon examining the substances collected, I found their appearance to be such as I have already described to you, namely, that of distinct globules, varying in size from that of a large pea to the bulk of a moderate grape, of a cream colour, and slightly translucent, of sufficient consistence to preserve their form, and to bear being cut by a knife, like soft wax, but at the points of their contact disposed to cohere. When exposed to heat, they readily melted, and then at once exhibited their original oily character. The change, which they have since experienced, has taken place in the water in which they have been kept.

"I am, dear sir,

"Yours always, very faithfully,

(Signed) "Wm. Babington."

To Sir Everard Home, Bart.

Our associate, Mr. Brande, afterwards examined the substance, and made the following report upon it:

"The globules voided appeared to be composed of the olive oil combined with mucus: the latter separated during putrefaction; and the oil was evolved, apparently unaltered. The relative proportion may be estimated at one-third animal matter, and two-thirds vegetable oil."

The following case, which was also communicated to me by Dr. Babington, shows that fat is sometimes formed in the intestines, and detected passing off with the fæces:

Elizabeth Ryder, four years and a half old, had been healthy for six months after her birth, when she became thin, had a sallow complexion, and was liable to jaundice. At a year and a half old, her belly was tumid, and she had great weakness in her back and limbs, for which complaints Dr. Babington was first consulted. At three years old, her mother observed something

come

come from her, as she walked across the room, which, when examined, was found to be fat in a liquid state, which concreted when cold. Ever since that time to the present, she has voided, at intervals of ten or fourteen days, the quantity of from one to three ounces, sometimes pure, at others mixed with fæces; when voided, it has an unusually yellow tinge, and is quite fluid like oil. Her appetite is good, as well as her spirits, and her flesh firm; her belly rather tumid, but not hard: she is subject to occasional griping: her urine natural, and she sleeps well. The specimen on the table was procured under circumstances which precluded all possibility of deception.

These facts, so strongly in favour of the opinion I had taken up, led me to devise in what way it might be put to the test of experiment. I tried to extract fat from the contents of the colon in different parts of its course, but without success. Failing in this mode of obtaining any decisive conclusions, I was led to believe the cæca of birds more favourable for experiments on this subject, and had those of a duck examined by Mr. W. Brande after the bird had been seven days without an evacuation. This confined state of the bowels put the parts nearly under the same circumstances as if they were in a diseased state. When the cæca were examined, they were found completely distended with fæces of the consistence of soft clay, so that, when the bags were laid open, the contents retained the same form. The intestine immediately above the cæca was empty, but the rectum was much distended; its contents were of a softer consistence than those of the cæca.

The following is Mr. Brande's report on this subject:

"The contents of the cæca were divided into two portions, of one drachm each, and comparative experiments were made with similar quantities of the contents of the rectum.

"*Exp.* 1. One drachm of the contents of the cæcum was completely immersed in half an ounce of water, and kept for seven days in a temperature varying from 40° to 60°. At the end of that time, warm water was poured upon it, but no appearance of fat could be perceived.

"*Exp.* 2. The same quantity of the contents of the cæcum was immersed in water containing one-fifteenth part of nitric acid, and kept under the same circumstances as in the former experiment. In seven days, warm water poured upon it separated a portion of oily matter, which concreted when cold, and appeared to be one-eighth of the whole mass.

"*Exp.* 3. Portions of the contents of the rectum were treated in the same way as in Experiments 1 and 2. That in water became putrid very rapidly, and showed no appearance of fat. The other, in the diluted nitric acid, was more dissolved than in

Experiment 2. Considerable extrications of gas took place, but there was no appearance of fat.

From these experiments, we learn that the contents of the cæcum, being confined there for some days, are in a state readily to be converted into fat by nitric acid, while the contents of the rectum are not, probably from being too putrid.

While engaged in this inquiry, I received from Sir Joseph Banks the carcase of a wild swan, which the Hon. Mr. Pelham had shot in the neighbourhood of the Humber. On examining the cæca, their contents were found to be of a bright green colour. This led me to propose to Mr. Brande to ascertain by experiment, whether an admixture of bile had any effect upon the process of converting animal substance into fat. The following experiments were made by Mr. Brande upon this subject:

Exp. 1. He took two portions of human muscle of the same size, and digested one of them in human bile, the other in water, both placed in the temperature of 100°.—In the first day the muscle in the bile underwent no change. On the second day it became soft in its texture, and had a slightly fetid smell. On the third day it became more fetid and yellow. On the fourth it had the smell of excrement, was flabby, very offensive, and fatty upon the surface. The portion of muscle, digested in water, had undergone no other change in the four days, but becoming slightly putrid, and there was no appearance of fat whatever.

Exp. 2. A similar experiment to that with the human bile, was made with a small portion of beef, and ox's bile, and the results were exactly similar.

Exp. 3. The last experiment was repeated in the temperature of 60°. In four days the beef became slightly fetid, and of a yellow colour; in six days it became more fetid, but there was no appearance of fatty matter.

Exp. 4. A portion of beef cut into pieces was digested in ox's bile, at the temperature of 100°. At the end of the fourth day the putrefaction was more advanced than in Experiment 2; the beef was washed and heated upon paper, but no greasy stain was produced.

From these experiments we learn, that the bile has a power of converting animal substance into fat; and that the temperature of 100°, or nearly so, is necessary for that process. We learn also, that this change is produced just as putrefaction is beginning to take place; and if the substance goes rapidly into putrefaction, no fat is formed; and, what is deserving of observation, the peculiar smell belonging to fæces, so different from that of putrid matter, is produced at the time that fat is procured.

Having succeeded in changing animal matter into fat, by

adding

adding bile to it out of the body, I was desirous of ascertaining whether this process could be detected going in in the human intestines; and being in attendance upon a gentleman of an advanced age, who had been six days without an evacuation from the bowels, confined to bed by the gout, I did not let slip the opportunity of his having a very costive stool deeply tinged with bile, to make the experiment. The excrement was put into water, and kept heated for three hours to a temperature of above 100°. When the water was allowed to cool, a film was formed upon the surface, which appeared to be of an oily nature, and Mr. Brande ascertained it to be so. The quantity was not great, but quite sufficient to ascertain the fact; and next day the fæces having subsided, the fatty film was much more conspicuous. In the Phil. Trans. for 1673, p. 6093, a case is stated of a person who laboured under an indisposition, attended with sickness and vomiting. In one attack of vomiting, he brought up matter resembling tallow, four pieces of which weighed half an ounce.

This process of forming fat in the lower intestines by means of bile, throws considerable light upon the nourishment derived from clysters, a fact well ascertained, but which could not be explained. It also accounts for the wasting of the body, which so invariably attends upon all complaints of the lower bowels. It accounts, too, for all the varieties in the turns of the colon, which we meet with in so great a degree in different animals. This property of the bile explains likewise the formation of fatty concretions in the gall-bladder, so commonly met with, and which, from these experiments, appear to be produced by the action of the bile on the mucus secreted in the gall-bladder: and it enables us to understand the following effects, which arose from the circumstance of no part of the bile passing into the intestines.

A child was born, at the full time, of the usual size, and lived for several months, but never appeared to increase in size, although it fed heartily, had regular stools, and the food seemed perfectly digested. There was no bile in the stools, and the skin was of a dark yellowish brown. I saw the child while it was alive, and was struck with its want of growth, and its having no fat under the skin, which made it appear longer than new-born children generally are. Upon examining the body after death, the only mal-formation met with, was there being no gall-bladder, nor any duct leading from the liver into the duodenum.

From what happened in this case, a supply of fat appears necessary for growth; for the child was by no means wasted in its muscles, which it must have been had the constitution not been supplied with nourishment.

Animal

Animal fat has, I believe, hitherto been considered as a secretion, although there is no direct evidence in favour of such an opinion. It has nothing in common with the secretions; it is met with in all the interstices of the body; is very often quickly deposited, and in as short a time taken back into the constitution. In these respects it corresponds with the watery fluids, with which the body is supplied.

In a former communication respecting the stomachs of animals, I explained that water was taken up from the stomach by channels yet unknown, and carried into the circulation; from whence it is poured into all the cavities of the body, or thrown out altogether by the kidneys and glands of the skin.

On the present occasion, I hope that I have collected a sufficient body of evidence to prove, that fat is formed in the intestines, and from thence received into the circulation, and deposited in almost every part of the body. When there is a great demand for it, as in youth, for carrying on the growth of the body, it is laid immediately under the skin, or in the neighbourhood of the abdomen: when not likely to be wanted, as in old age, it is deposited in the interstices of muscles, to make up in bulk for the wasting of the muscular fibres. There appear to be no direct channels by which any superabundance of it can be thrown out of the body; so that, when the supply exceeds the consumption, its accumulation becomes a disease, and frequently a very distressing one.

X. *An Attempt to determine the definite and simple Proportions, in which the constituent Parts of unorganic Substances are united with each other.* *By* JACOB BERZELIUS, *Professor of Medicine and Pharmacy, and M.R.A. Stockholm.*

[Continued from vol. xlii. p. 463.]

THIRD CONTINUATION.

Containing the laws of the combinations of water, and of the formation of subsalts and double salts, together with the results of the whole investigation.

I HAVE expressed an opinion in the former parts of this Essay, that water takes place of a base with respect to the acids, and of an acid with respect to the bases; I have also advanced as a probable supposition, that the salts contain always so much water of crystallization, that its oxygen is either an integral multiple or submultiple of that which is contained in the base of the salt. I had inferred from Davy's experiments, that the gaseous muriatic

tic acid contains such a quantity of water as is sufficient for oxidating with its oxygen as much metal as is capable of producing a neutral salt with the dry muriatic acid; and Gay-Lussac has proved the same thing in his excellent essay on the combinations of the gases. I had also found by experiment that the sulphuric acid could only be concentrated by boiling, until the remaining water contained one-third as much oxygen as the acid, that is, as much as would be required by the same acid in a base capable of saturating it. I had assumed the component parts of water $11\frac{3}{4}$ of hydrogen, and $88\frac{1}{4}$ of oxygen, from my own experiments, confirmed by those of Biot and Arago. Mr. Gay-Lussac makes them $13\frac{1}{4}$ and $86\frac{3}{4}$, without altering the determination of the specific gravity of the gases. I shall endeavour to throw a clearer light on these combinations of water by the experiments now to be described. I ought perhaps to have been deterred from making some of them public by the difficulty of the subject, and the impossibility of fully establishing my opinions respecting the composition of hydrogen; but science would often have profited by communications which have been withheld by too great a desire of minute accuracy.

I shall here make some preliminary remarks on subjects connected with the immediate objects of my pursuit; and first, respecting the *impossibility of exhibiting* some *of the acids in a separate state*. We have learned from the excellent experiments of MM. Gay-Lussac and Thenard, that the muriatic acid cannot be separated from the substances with which it is combined, unless in the presence of water, with which it may combine at the moment of its formation. Mr. Davy's inferences from this fact are well known; but we shall here pause a little to examine the affections of other acids in similar circumstances.

Sulphur, as we have reason to believe, is capable of four degrees of oxygenization. The first and second are unknown except in combination with the muriatic acid; the third is the sulphurous acid, and the fourth the sulphuric. No chemist has ever obtained dry sulphuric acid, and whoever attempts to procure it, will find sulphurous acid in its place. If, for example, we burn sulphur in perfectly dry oxygen, a very slight trace only of sulphuric acid is formed, and this originates from the hydrogen present in the sulphur, which affords a little water. If we dry sulphate of the oxide of iron, or alum, with proper care, and attempt to drive off the sulphuric acid by heat, we obtain oxygen and sulphurous acid, without the condensation of a trace of sulphuric acid; but if we cause aqueous vapour to pass over these salts when heated, we immediately obtain sulphuric acid condensed in the receiver. It is known on the other hand, that if we throw sulphuretted muriatic acid into water, sulphurous acid

is disengaged, and a precipitate of revived sulphur appears. We may therefore safely assume, that of the four stages of oxygenization of sulphur, only one is capable of being separately exhibited.

Nor can the *nitric* acid be obtained without water. If we attempt it, we get nothing but oxygen gas and nitrous acid. But when these gases come into contact with each other over water, the affinity between the substances, which had been suspended, is restored, and a compound of nitric acid and water is formed. Consequently the nitric acid cannot exist alone, but requires the presence of a third body containing oxygen. If the nitric acid were incapable of being exhibited in a lower degree of oxygenization, and if the affinity of oxygen to its radical were very strong, it would obviously be perfectly impossible to expel this acid from any of its combinations by means of a dry, although a stronger, acid, with the assistance of heat; for in this case two strong affinities would oppose the one which tends to the formation of the dry acid; that is, the attraction of the nitric acid to the base of the combination, and that of the oxygen to the radical of the acid, which must both be overcome in order that the separation may take place.

But it often happens that the attraction of a radical to oxygen is incomparably greater than that of an acid to a base. It is therefore very natural, that an acid which cannot be exhibited in a separate state, and of which the radical has a very strong attraction for oxygen, should be incapable of being separated from its combinations without the intervention of water, or of some other oxidated body.

This is precisely the case with the *muriatic acid*, and it possesses the properties which have led the acute Mr. Davy to his peculiar opinions respecting this acid, in common with the sulphuric, the nitric, and, as we shall see hereafter, with several other acids. The experiments, which have been performed, in order to the decomposition of the muriatic acid, prove that the radical of this acid has a remarkably powerful attraction for oxygen, if not the greatest of all existing substances. But if we attempt to decompose ignited muriate of soda with fused boracic acid in a red heat, we oppose the weak attraction between the boracic acid and the alkali to the much stronger attraction of the muriatic acid for the alkali, and the still more powerful affinity of the radical of the acid to oxygen: hence it is very obvious that no separation of the muriatic acid from the base can take place.

When the celebrated Mr. Davy entertained hopes of decomposing the *oxymuriatic acid gas* by means of charcoal, which he kept ignited in it for an hour, he seemed to suppose that the carbon had a greater affinity for oxygen than the radical of the

muriatic

muriatic acid. For, if it is impossible to exhibit the muriatic acid in a separate state, it would have been requisite that the carbon should separate from the radical not only the small portion of oxygen which distinguishes oxymuriatic from muriatic acid, but the whole quantity that exists in the combination. Now the attraction of carbon for oxygen is much weaker than that of the muriatic radical, and the experiment cannot be admitted as a valid proof that the oxymuriatic acid contains no oxygen.

A second preliminary remark relates to the *water of crystallization of salts.* If we examine a solid that contains water, we must not forget that the water may be either in a state of chemical combination, or merely retained mechanically in the pores of an aggregate. The internal parts of crystals are not completely continuous, but interrupted by very small irregular interstices, occupied by a small portion of the mother liquor, in which the crystals have been formed. For this reason, regular saline crystals, although they derive their origin from a power which detaches them from all chemical connexion with the substances remaining in solution, still contain a small quantity of the foreign substances which had been present, however carefully their surfaces may have been washed. Crude saltpetre, for instance, crystallized from the first solution, exhibits yellowish-brown crystals. But this saltpetre is not chemically combined either with the colouring extractive matter, or with the alkaline muriate, which is found in it. By the help of a magnifying glass we see clearly that the white salt contains a yellow substance in its pores; and the same must be true of the impurities which are not distinguishable by their colour. The less pure the mother liquor, the more must the crystals be loaded with foreign substances; and the purification by repeated crystallizations depends on the mechanical admixture of a mother liquor which becomes less and less charged with impurities at each step of the process. A large crystal contains in its pores a quantity of the mother liquor proportionally larger than a small crystal. Thus, in the refineries of sugar, the syrup which is separated during the continual agitation that is employed for obtaining lump sugar, by crystallization in small grains, is commonly more coloured than the syrup from which sugar candy has been separated during a slow evaporation, in large yellow or even brown crystals. In the same manner it has been usual to purify nitre for fireworks by causing it to precipitate in fine grains; and we know from the experiments of MM. Thenard and Roard, that alum may easily be purified by a similar process.

If we powder a salt that has been crystallized in very small grains, and well dried, it affords immediately a dusty dry powder. If,

If, on the other hand, we attempt to pulverise the same salt equally well dried, but consisting of larger crystals, we obtain a damp powder, somewhat cohering, from containing a portion of the mother liquor, that oozes out of the pores, and causes the appearance of moisture. If we dry this again, we may then reduce it to a state of dust; except in the case of particular salts, which exhibit some degree of cohesion even when perfectly dry.

The water mechanically included in crystals is generally expelled by the effect of heat, together with the water of crystallization; but in the salts which contain no water of crystallization, and which are exposed in large masses to a high temperature, the water is suddenly changed into vapour, and causes the salt to decrepitate. Consequently the water thus expelled cannot have been water of crystallization, since it must have been unequally distributed through the crystal,—a condition incompatible with chemical combination. For this reason, the estimates of the quantity of the supposed water of crystallization in decrepitating salts have been extremely various, according to the magnitude of the crystals examined, the larger crystals affording the greater proportion.

That decrepitating salts might contain water of crystallization is not altogether impossible, for the water of crystallization might be expelled at a higher temperature than that which produces decrepitation: but as far as we have hitherto examined, the combinations which retain water the most obstinately are always fused at a moderate temperature, and the water which is only united mechanically to the crystals is expelled by boiling.

I have premised these observations, in order to show how difficult it may be to determine the quantity of the water of crystallization with perfect accuracy. But hereafter this determination will be rendered very easy: for, when the law of the water of crystallization is once determined, some experimental approximations only will be required, in order to determine among the different suppositions which can possibly be admissible: and this law I imagine that I have established in the experiments which are now to be related. I have generally employed granular crystallizations; and if the salt was not much disposed to crumble, I have rubbed it into a fine powder, suffered it to remain exposed for some hours to dry air, and then heated it in a crucible of platina, of which the weight, as usual, had been accurately determined. I have also examined different forms of crystallization of the same salt, but have been unable to find any difference in the quantity of water contained in them.

The principal subjects of the present division of my essay are four: *first*, Laws for the combinations of water with acids, bases,

bases, and salts; *secondly*, Laws for the formation of subsalts; *thirdly*, Laws for the formation of double salts; and *fourthly*, General view of the results of my experiments.

I. Laws for the Combinations of Water.

A. *Combinations of Water with Acids.*

1. *Tartaric Acid.*

Ten grammes of finely powdered dry tartaric acid were dissolved in water, and heated with a solution of acetate of the protoxide of lead, as long as any precipitate was formed. The mixture was evaporated to dryness, and the uncombined acetic acid was in great measure expelled. The mass that remained, when well washed on a filter, afforded 23·51 gr. of tartrate of protoxide of lead.

Five grammes of this substance, when treated with dilute sulphuric acid, afforded, in three different experiments, 4·2318, 4·229, and 4·228 gr. of sulphate. Consequently the tartrate must consist of

Tartaric acid	37·75	100·00
Protoxide of lead ...	62·25	164·87

Hence the 23·51 gr. of tartrate must contain 8·875 of dry acid, and 100 parts of the crystallized acid contain 11·25 of water, and 100 of dry acid unite with 12·7 of water, which contain 11·2 of oxygen. But the protoxide of lead saturated by 100 parts of this acid contains 11·788 parts of oxygen; so that, notwithstanding the slight difference in the quantities, it is pretty obvious that the crystallized tartaric acid must contain a quantity of water, in which there is as much oxygen as in the base saturated by the acid. In the experiments which I have undertaken, in order to investigate the laws of the formation of organic productions, and which I shall hereafter make public, I have only found in the tartaric acid 56·384 per cent. of oxygen, with 39·206 of carbon, and 4·41 of hydrogen, which is nearly five times as much as is contained in the water that saturates 100 parts of tartaric acid. In two experiments on the tartrate of the protoxide of lead, I obtained from 3 grammes, with a very slight variation, 3·7555 gr. of carbonate of lime, and ·425 of water. If now the compositions of this tartrate, of the carbonate of lime, and of water, are correctly ascertained, the deficiency in the computation must be made up with oxygen: but it must be allowed that this determination is subject, from various causes, to an error of as much as 1 per cent. I must here repeat the remark, that I may possibly have estimated the quantity of oxygen contained in the different bases somewhat too high, and that hence may arise the difference of the quantity as deduced from the

the oxygen of the acids and that which has been more directly determined. But I have endeavoured to distribute this error as equally as possible through all the results, so that the first perfectly correct and well-established analysis of an acid or an oxide may correct the whole at once.

2. *Citric Acid.*

Some crystallized citric acid was finely powdered, and dried for some hours in the sun.

a.) Five grammes of this acid were dissolved in water, and mixed with 15 gr. of levigated protoxide of lead, which had been ignited the instant before; the mixture was slowly evaporated to dryness, and the salt of lead, mixed with the protoxide, was then dried, in as strong a heat as it would bear, on a sand bath, until it lost no more of its weight. The mixture now weighed 18·95 gr. Consequently the acid had contained 21 per cent. of water.

b.) Ten grammes of the same citric acid, treated in the same way as the tartaric, afforded 23·756 gr. of the citrate of the protoxide of lead.

c.) Five grammes of the same citric acid, kept melted in a gentle heat, as long as they lost any part of their weight, were found to be deficient, when they were congealed into a solid mass, ·354 gr. or 7·08 per cent.

d.) Ten grammes of citrate of the protoxide of lead, treated with diluted sulphuric acid, gave 9·056 gr. of sulphate of the protoxide, in which are contained 6·666 gr. of the protoxide. Consequently this salt contains one third of its weight of acid, and consists of

Citric acid .	33·333	100
Protoxide of lead	66·667	200

Hence 100 parts of citric acid must saturate a quantity of the base which contains 14·3 of oxygen. If we compute from experiment *b.*), according to this analysis, we find again that the citric acid contains 20·85 per cent. of water. Now this acid had lost, in a temperature not high enough to decompose it, 7·08 per cent. of water; and this makes about a third of the whole that it contains. The remaining 13·77 parts of water were attached to the acid by a stronger attraction. But if 79·15 parts of dry citric acid contain 13·77 of water strongly attached to them, 100 parts of dry acid must combine with 17·14 of water, which contain 15 of oxygen, or a little more than is contained in the protoxide of lead which saturates 100 parts of the dry acid. This small difference depends on the impossibility of obtaining perfectly correct results from substances, which, like the citric acid, are easily decomposed in a temperature somewhat elevated.

elevated. Hence we see that the citric acid contains a quantity of water of crystallization, which may be separated from it without altering the composition of the acid, and that this water of crystallization is half as much as that which is more intimately combined with the acid.

3. *Oxalic Acid.*

It is a fact generally known, that the oxalic acid effloresces or falls into powder in dry air. I thought at first that it lost the whole of its water during this change; but upon exposure to a temperature somewhat above the boiling point of water, several portions of it were changed into coherent masses, but never lost exactly equal parts of their weight. The variations however were only from 28 to 29 and 29·3 per cent.; and in the last case it was obvious that a sublimation had commenced, for the surface of the salt was covered by small crystalline needles.

I now mixed in a small glass dish four grammes of powdered oxalic acid, which had been dried for an hour in the air, with 20 gr. of finely powdered protoxide of lead, which had just been ignited; I poured water on the mixture, and dried it rapidly, stirring it frequently in the mean time. The acid and the protoxide had now lost 1·68 gr. of their weight. I repeated the experiment with a result precisely similar. Hence 100 parts of oxalic acid contain 42 parts of water.

Ten grammes of well dried oxalate of the protoxide of lead were burnt in an open glass dish exposed to a red heat. The dish lost, in different experiments, from 2·52 to 2·53 gr. of its weight. The remaining protoxide had a shining yellow colour. Consequently the *oxalate of the protoxide of lead* consists of

Oxalic acid........	25·2	100·0
Protoxide of lead...	74·8	296·6

Hence 100 parts of oxalic acid saturate a quantity of a base of which the oxygen amounts to 21·2 parts. Now we have seen that 100 parts of crystallized oxalic acid contain 42 of water, of which they lose from 28 to 29 by exposure to the air. But 28 is exactly $\frac{2}{3}$ of 42; so that the oxalic acid retains 1 part of water for every two that it loses. Hence we find that 100 parts of dry oxalic acid unite with 72·414 of water, and lose 48·276 of these in drying. The remaining 24·138, which can only be driven off by the assistance of other substances possessed of stronger [positively] electrical powers with relation to it, contain 21·3 parts of oxygen.

4. *Results.*

We are therefore acquainted with none of the three vegetable acids, here analysed, in a separate state; and it may be presumed

 that,

that, like the mineral acids before mentioned, *they cannot be exhibited in a separate state.*

We have also seen, that both the citric and the oxalic acids contain a portion of water which may be expelled, and which may be considered as water of crystallization, while the acid remains combined with another portion of water, which completely assumes the place of a base, and can only be expelled by a stronger base. We may properly consider these combinations with water as salts, in which water is the base, and stands precisely in the predicament of the weaker bases among the metallic oxides, which are capable of combining both with acids and with other bases. If we consider the water which escapes from the oxalic acid during its efflorescence as water of crystallization, it contains twice as much oxygen, as the portion of the water, which is to be considered as the base of the combination.

It is not probable that these determinations can be liable to any error from the presence of water in the oxalate of the protoxide of lead: for from 5 gr. of well dried oxalate of lead, properly burnt in close vessels, I obtained in two experiments only from ·077 to ·09 gr. of water, together with a quantity of carbonic acid which afforded 3·708 of carbonate of lime. If we calculate the composition of the *oxalic acid* from the analysis of the oxalate of the protoxide of lead, the carbonate of lime, the carbonic acid, and water, this acid must consist of 34·9 of carbon, nearly 1 of hydrogen, and 64·1 of oxygen. But we have seen that 100 parts of oxalic acid unite with so much of a base as contains 21·2 parts of oxygen, and $21·2 \times 3 = 63·6$. We may therefore consider it as proved, that the oxalic acid must contain three times as much oxygen as the base by which it is saturated. Finally, we "have found, that" the water contained in the crystallized oxalic acid contains exactly as much oxygen as the acid itself.

It is true that these analyses of the vegetable acids differ considerably from those of Gay-Lussac and Thenard; probably on account of the water united with them. For, if we subtract from the acid of the tartrate of lime as much water as contains twice as much oxygen as the lime, we find the capacity of the acid for saturation such as is determined by my experiment on the tartrate of lead. The same is true of the citrate and oxalate of lime, if we deduct from them as much water as contains a quantity of oxygen equal to that of the lime. With this correction, the analysis of the oxalic acid agrees pretty well with mine; but there is still a considerable variation in our analyses of the tartaric acid; and time must decide which is the least inaccurate.

B. *Com-*

B. *Combinations of Water with Bases.*

1. *Alkalies and Alkaline Earths.*

The extrication of a quantity of hydrogen gas, during the reduction of the alkalies to a metallic form, by the action of iron raised to a white heat, has induced several chemists to endeavour to discover water in the fixed alkalies after ignition. The results of these investigations differ in some measure from each other, although they evidently oscillate about the same point.

Since it is perhaps impossible to obtain a perfectly pure dry caustic alkali, which contains neither carbonic acid, nor earth, nor any other foreign substance, so that experiments upon such an alkali can afford no very accurate result, I chose for these investigations lime and magnesia, convinced that what is true of one of these stronger bases, must also be applicable to another.

Lime.

Some pure well-burnt lime was slaked with water, and strongly ignited again in a platina crucible, so that all the carbonic acid left in the lime might be expelled with the aqueous vapour. *a.*) Of this caustic lime 10 grammes were slaked with water, dried as soon as possible, and lastly heated over a spirit lamp far above the boiling point of water. They had acquired an additional weight of 3·21 gr. *b.*) The experiment was repeated with 30 grammes of pure lime, and afforded 39·75 gr. of slaked lime.

Consequently in both these experiments 100 parts of lime had taken up from 32·1 to 32·5 of water. Since in the latter experiment the greater bulk of the lime required a longer time for drying, it is obvious that the weight must have been increased by the absorption of some carbonic acid. But 100 parts of lime contain 28·16 of oxygen, and 32·1 of water 28·5. The slight difference evidently depends on the absorption of carbonic acid while the lime is drying: and the experiments sufficiently prove, that the lime takes up a quantity of water, of which the oxygen is equal to that of the earth.

Magnesia.

a.) In order to ascertain the quantity of oxygen contained in magnesia, I expelled the water of crystallization of some pure sulphate of magnesia by heat, dissolved the salt in water, after slightly igniting it, and precipitated the sulphuric acid with muriate of baryta. Ten grammes of ignited sulphate of magnesia afforded 19·43 of ignited sulphate of baryta, answering to 6·664 of sulphuric acid.

b.) I took five grammes of caustic magnesia, which had been prepared from the carbonate, precipitated with pure carbonate

 of

of potass from a boiling solution of sulphate of magnesia, observing the precaution that all the magnesia should not be precipitated, since otherwise the last portions would contain some carbonate of potass, which could not be washed away: I dissolved them in diluted sulphuric acid, evaporated the solution, and ignited the residuum in a platina crucible: they left 14·742 gr. of sulphate, which, when redissolved, left behind a slight trace of magnesia, and had consequently lost a small portion of their acid by the effect of the heat. According to this experiment, the dry salt contains 66·1 parts of sulphuric acid in 100.

Although it is possible that this experiment may have been more accurate than the former, I shall however prefer that as a ground of computation, because the errors to which it is liable must have proportionally affected the other comparative experiments. We have therefore for the sulphate of magnesia

Sulphuric acid	66·64	100·00
Magnesia	33·36	50·06

Consequently, according to this calculation, we have 39·872 parts of oxygen in 100 of magnesia. According to Experiment *b*) they contain only 38·8. Mr. Hisinger found, by means of the decomposition of the muriate of magnesia with the nitrate of the protoxide of silver, 38·3 parts of oxygen in 100 of magnesia.

Ten grammes of caustic magnesia, moistened with water, and dried in a platina crucible, afforded, in different experiments, when heated far above the boiling point of water by means of a spirit lamp, 14·25, 14·35, and 14·4 grammes of the combination of the earth with water. Now 4·4 gr. of water contain 3·883 of oxygen: consequently this agrees very accurately with the result deduced from experiment *b*). I do not however venture to declare this experiment the more accurate, since possibly a part of the magnesia may have been deprived of its water in it, at the bottom of the crucible, where the flame touched it. For when the magnesia, which to all appearance was dry, was placed over the lamp, it lost very rapidly a considerable quantity of water, until about 14·6 gr. only were left, and then half an hour was required in order to reduce it to 14·4 gr. or less. Consequently these experiments cannot be perfectly accurate; but we collect very clearly from them that magnesia, like lime, is capable of combining with a quantity of water, of which the oxygen is equal to its own.

Conclusions.

I consider these examples as affording a sufficient foundation for the supposition that the same law prevails with respect to the fixed alkalies and the other alkaline earths. But if these hold

hold in combination a quantity of water, of which the oxygen is equal to that of the earth; and if they are capable of taking up fresh quantities of water which correspond to the water of crystallization, as they are known to do, they must contain water in two different states, like the acids which have already been described. One of the portions of water corresponds, in comparison with these stronger bases, to a [negative] body, or an acid, and cannot be expelled from its combination with the fixed alkalies, with baryta, and with strontia, by means of ignition, but only by the action of a body which is either intrinsically more strongly [negative], or which assumes that character at the given temperature.

If we compare the experiments of Berthollet, Davy, and Darcet, on the quantity of water contained in fused potass, we find that they differ little from 16·15 per cent.; and since this is exactly the quantity of water which contains equal quantities of oxygen with the potass, these experiments may be considered as a proof of the opinion which I have here advanced. We know from the able experiments of Mr. Bucholz, that crystallized baryta loses half its weight in the fire; it still remains fluid, and can be deprived of no more water by continued ignition; but Bucholz and Gehlen have shown more lately, that even in this state it still contains water. According to the analogy of lime, we should expect this to amount to 10·59 per cent. If then 200 parts of crystallized baryta lose about 100 of water in the fire, the water of crystallization here amounts to either nine or ten times as much as that which is combined in the place of an acid.

The property of the fixed alkalies and alkaline earths, which enables them to retain water at a very high temperature, may afford us the means of attaining an object of great importance to the whole theory of chemical and physical science, a comparison of the force of chemical attraction with common mechanical force. We know how immense a force is required to retain water in a state of liquidity at elevated temperatures: probably no vessel of iron can be made strong enough to contain it at the heat of ignition, while this effect is actually produced by the attraction of the alkalies and of baryta to water. If the force which is required for such an effect were expressed by the height of a column of quicksilver, we should at least obtain a very high numerical estimate of this force, which is exceeded by the attraction of these bodies to water; but we should not obtain a correct measure of the attraction, since, at a still higher temperature, both the alkali and the water are raised in vapour. But lime and magnesia would allow of such a comparative estimate, and for these substances we might determine the height of a co-

lumn of quicksilver, under the pressure of which aqueous vapour would begin to condense at the temperature required for expelling the water out of these earthy hydrates: and this height would express the mutual affinity of the alkaline earths and water, and might be compared at pleasure with any other mechanical power. And since chemical attractions may be compared with each other, we might perhaps by degrees advance so far by these means, as to be able to express every chemical affinity in numbers, and to compare it with gravity, the universal measure of mechanical force.

We shall hereafter see that some of the weaker bases, when they take up water, retain it with a force so moderate, as to be exceeded by the expansive power of the water even at lower temperatures. Some of them thus become in some measure hygroscopic substances, since their greater or less approach to the maximum of moisture depends on the dryness of the air, and only takes place when the air exhibits a similar maximum, and the expansive power of the water at the given temperature is completely annihilated.

[To be continued.]

XI. *The Discovery of the Atomic Theory claimed for Mr.* HIGGINS. *By* JOHN NASH, *Esq.*

Merrion Square, Dublin, Jan. 7, 1814.

SIR, I BEG leave to call the attention of your readers to a publication which has recently appeared in a cotemporary Journal, and to point out a mis-statement there given to the public, injurious, in my opinion, to the scientific reputation of a learned and respected individual.

The passage I allude to is contained in No. 12 of Dr. Thomson's Annals of Philosophy, and is in these words:

"When we reflect on this cause, it appears at first evident that it must be of a mechanical nature; and what presents itself as the most probable idea, most conformable to our experience, is, that bodies are composed of atoms, or of molecules which combine 1 with 1, 1 with 2, or 3, 4, &c.; and the laws of chemical proportions seem to result from this with such clearness and evidence, that it seems very singular that an idea so simple and probable has not only not been adopted, but not even proposed before our own days. As far as I know, the English philosopher Mr. John Dalton, guided by the experiments of Bergman, Richter, Wenszel, Berthollet, Proust, and others, was the first person who endeavoured to establish that hypothesis. Sir Humphry Davy has lately assured us, that Mr. Higgins in a book published in the year 1789 established the same hypothesis. I have not seen the

the work of Mr. Higgins, and can only notice the circumstance on the authority of Davy."

To which the learned Editor has subjoined a note of his own in the following words:

"The work of Higgins on *Phlogiston* is certainly possessed of much merit, and anticipated some of the most striking subsequent discoveries. But, when he wrote, metallic oxides were so little known, and so few exact analyses existed, that it was not possible to be acquainted with the grand fact that oxygen, &c. always unite in determinate proportions which are multiples of the minimum proportion. The atomic theory was taught by Bergman, Cullen, Black, &c., just as far as it was by Higgins. The latter indeed states some striking facts respecting the gases, and anticipated Gay-Lussac's theory of volumes; but Mr. Dalton first generalized the doctrine, and thought of determining the weight of atoms of bodies. He showed me his table of symbols, and the weight of the atoms of six or eight bodies, in 1804; and, I believe, the same year explained the subject in London, in a course of lectures delivered in the Royal Institution. The subject could scarcely be broached sooner. But about the same time several other persons had been struck with the numbers in my table of metallic oxides published in my *Chemistry;* and the doctrine would have certainly been started by others if Dalton had missed it."

That learned Editor by this publication has endeavoured to deprive Mr. Higgins of the honour due to the first author of the atomic theory, a doctrine now so generally received and universally admired, and of which Sir H. Davy, Berzelius, and others have spoken in terms of unqualified approbation.

Dr. Thomson has in a part of his note endeavoured to bestow the credit of that theory upon Mr. Dalton, and in another part to fritter away the merits and importance of the discovery. And that the discovery is important, and that merit is due to the author, I think, is fully established by the opinion of such men as I have mentioned.

I shall now proceed to show, by reference to dates and facts, that the merit of that discovery exclusively belongs to Mr. Higgins, and that Dr. Thomson was not justifiable in making the assertions in his note.

It is well known, that for several years the attention of the chemical world was directed to the consideration of the phlogistic and antiphlogistic doctrines. Whilst these questions were agitated by their respective advocates, Mr. Higgins, without adopting either the one theory or the other, commenced, upon the true ground of experiment and analysis, to examine the foundation of both, and the atomic theory was one result of that investigation.

vestigation. And accordingly Mr. Higgins, in his work printed in the year 1790, gave that theory to the world, ten years, at least, before either Mr. Dalton or his learned panegyrist even published a hint of such a doctrine. That work which Mr. Higgins styled *A comparative View of the Phlogistic and Antiphlogistic Theories*, and which Dr. Thomson calls (and I think disingenuously) a work upon Phlogiston, was very generally read, and is now to be found in the library of every scientific society and individual, with that date for its publication prefixed to it.

Reference to dates establishes priority; and reference to the works of Mr. Higgins and Mr. Dalton will fully establish this, that Mr. Higgins left the atomic theory fully as perfect as Mr. Dalton; and I would with confidence call upon the most zealous advocate of the latter gentleman, to show in his work any one position, or principle, which is not founded upon, or deducible from, the doctrine first discovered and established by Mr. Higgins.

Dr. Thomson in the note alluded to asserts, that "*when Mr. Higgins wrote, metallic oxides were so little known, and so few exact analyses existed, that it was impossible to be acquainted with the grand fact, that oxygen, &c. always unites in determinate proportions which are multiples of the minimum proportion.*" To set the Doctor right on that head, I will beg leave to refer him and the reader again to the Comparative View, page 295 &c. where they will find that treated of which the Doctor asserts to be then unknown; and what he calls the *grand fact*, will be found to be the greatest leading principle which Mr. Higgins endeavours to establish in that work, particularly in that part where the molecules of different acids are represented by diagrams, with their respective number of particles of oxygen and bases; and it was that which first gave a clear idea of definite proportions.

I apprehend the Doctor is not more fortunate in the assertion, "*that the atomic theory was taught by Bergman, Cullen, Black, &c. just as far as by Higgins.*" Now I have looked over carefully the works of those chemists, and also an accurate manuscript note or report of Black's Lectures; and I affirm, there is not in any of them the slightest mention of the atomic theory. Indeed I must here remark, that the Doctor has not attempted to support any of his assertions by quotations: it was prudent not to make the attempt, he could not succeed.

In contradiction to what the Doctor says, "*Mr. Dalton first generalised the doctrine, and thought of the weight of atoms of bodies,*" I need only refer the reader to Mr. Higgins's work, pages 15, 37, and particularly to pages 80 and 81. As to that remark of the Doctor's with which he closes his note, that "*the doctrine would be started by others if Dalton had missed it,*" I must

must say that it is most disingenuous: first, because it indirectly makes Mr. Dalton the starter (to use the Doctor's metaphor) of that whereof he was only the pursuer, and when it appears, and that to the Doctor's knowledge, that Higgins put up the game; and next, because that sentence conveys a sentiment unworthy of any scientific man, equally disparaging to merit of any discovery whatsoever, the best answer to which is to remind the Doctor of the story of Columbus and the egg.

I wish it to be clearly understood, that I by no means attempt to attribute the learned Doctor's mis-statement to any unworthy motive; nor is it any part of my intention to enter into a vindication, or explanation, of the theories and the positions laid down in the production of Mr. Higgins. Such were I even capable of performing would now be superfluous, as I understand there is at present in the press a work on that subject, from the pen of the learned Professor himself. I mean no more than to claim for Mr. Higgins the merit of being the original author and promulgator of the Atomic Theory.

I am, sir,

Your obedient humble servant,

To Mr. Tilloch. JOHN NASH.

XII. *Preparation of the lately discovered new Substance called* IODE, *which possesses the singular Property of becoming converted into a beautiful violet-coloured Gas by the mere Application of Heat. Communicated by Mr.* FREDRICK ACCUM *in a Letter to the Editors.*

SIRS, YOU will receive with these lines a small glass tube, containing a specimen of the extraordinary substance lately discovered in France, which possesses the singular property of becoming converted into a beautiful violet-coloured gas, by the mere application of a gentle heat, and condensing again unaltered into a solid state, resembling plumbago or black lead, when suffered to grow cold.

As this substance, to which the name of iode has been given, has within these few weeks arrested the attention of chemists, and as the mode of obtaining it has not yet been published in this country, I take this opportunity of stating, that it may be procured by distilling, with a very gentle heat, the uncrystallizable saline mass which is obtained, or left behind, after separating all the crystallizable salts from a lixivium or solution of kelp, or Spanish barilla of commerce.

For the purpose of experiment or exhibition in a Lecture-room, the following easy process answers exceedingly well:

Take

Take a thin glass tube about 10 or 12 inches long, and 3-8ths of an inch in the bore; put into it about one drachm of the uncrystallizable residue before mentioned, previously fused for a few minutes, to free it as much as possible from water, and reduced to a coarse powder: add to it, without soiling the inside of the tube*, about half its weight of concentrated sulphuric acid: shake the whole together, and apply a gentle heat, by means of a taper or lamp. This being done, a dense white vapour will make its appearance, and a black glistening powder, which is iode, become sublimed in the colder part of the tube. Then cut to a convenient length, with a file, that part of the tube which contains the iode, and seal the extremities of it by means of the blow-pipe or spirit-lamp.

The preparation of iode upon a larger scale is equally simple and easy. Let a long slender-necked tubulated retort be placed in a sand-bath; surround the whole body of the retort up to the tubulure with sand, and adapt, without luting, to the beak of it, a wide-mouthed phial or receiver. This being done, introduce through the tubulure, first, one part of sulphuric acid, and then two parts of the saline mass, before mentioned, broken into small pieces of the size of split pease, and distil for a few minutes with a gentle heat. The iode will become sublimed into the neck of the retort in a crystalline form, exhibiting a black shining crust. Cut off the neck of the retort with a file, and collect the iode by means of a feather or camel's hair brush.

If the whole of the saline mass of kelp or barilla, freed from carbonate of soda only, and which of course consists of muriate of soda, muriate of potash, sulphate of potash, hydrosulphuret of potash, &c. be treated with sulphuric acid, the preparation of iode becomes more embarrassing and difficult.

I have the honour to be, yours,

Compton Street, Soho. FREDRICK ACCUM.

XIII. *Account of the new Substance discovered by M.* COURTOIS, *and called* IODE†.

THE new substance to which we may give the name of *iode*, possesses in a high degree the electric properties of oxygen, and oxygenated muriatic acid. When it is purified by means of potash and distillation, it is infusible at the temperature of boil-

* This may be done conveniently, by sucking the acid up with the mouth into a long small glass tube drawn out to a capillary point, applying the finger to the upper orifice of it, and thus transferring by means of it the acid into the larger tube.

† Translated from the Paris *Moniteur* of Dec. 12, 1813.

ing

ing water, and has nearly the same volatility as this fluid. By no chemical agent does it offer any trace of muriatic acid. The iode combines with nearly all the metals; but as it is solid, it does not appear to develop in its combinations so much heat as the oxygenated muriatic acid, with which it has much analogy in its general properties. To give an idea of its relations with other bodies, we shall compare it with this acid, applying also the two hypotheses which have been formed respecting its nature, and adding, that on combining with hydrogen it forms a peculiar very strong acid, which can be procured in the gaseous state, which is very soluble in water, and which has the same relation to iode that muriatic acid has to oxygenated muriatic acid or chlorine. The action of phosphorus on iode furnishes the means of obtaining the new acid in both its states, gaseous and liquid.

If we agitate iode and phosphorus together, both perfectly dry, we obtain a reddish-brown substance which emits no gas; if we moisten this substance, it immediately yields copious fumes very acid, and at the same time phosphorous acid is formed. We can easily obtain the new acid in a gaseous state, by using the iode a little moistened; there is then enough of water to assist its formation, but not sufficient to condense it. If we combine the iode and phosphorus under water, only a very small quantity of subphosphuretted hydrogen gas is disengaged, and the water becomes very acid: if the new substance be in excess, the liquid is deeply tinctured of a reddish-brown; on the contrary, it is colourless if the phosphorus predominates. It generally remains in a mass of a red colour, insoluble in water, and in which we find some phosphorus and iode. Nevertheless, their proportions may be such that we can obtain no residuum, and the liquid may be as limpid as water. If we submit this acid liquor to distillation, water comes over, and the new acid does not pass into the receiver till the liquid in the retort is highly concentrated: in the latter, pure phosphorous acid remains, which soon yields an abundance of phosphuretted hydrogen gas. Thus, when phosphorus and iode are dry, a combination analogous to that of oxygenated muriatic acid with phosphorus is formed; and when they are moistened, the same phænomenon is produced as with the liquor of phosphorus poured into the water, while the oxygen of the latter with the phosphorus forms phosphorous acid, its hydrogen combining with the iode to form the new acid.

We now see the characters of this acid: in the gaseous state it is colourless, smelling nearly like muriatic gas, smoking when in contact with the air, rapidly absorbable by water, giving with the oxymuriatic gas a fine purple vapour, and speedily changing over mercury: it forms with this metal a greenish yellow substance, similar to that which we obtain directly with mercury and the vapour

vapour of iode, and it produces hydrogen gas equal in volume to the half of the acid gas. A few minutes agitation are sufficient to decompose it entirely. Iron and zinc produce a similar effect.

This acid in the liquid state, obtained by dissolving the gas in water, forms, as is said above, a very dense liquid not very volatile: it rapidly decomposes the carbonates, and dissolves iron and zinc with an extrication of hydrogen gas: but it does not attack mercury even when warm; which proves that it has a strong affinity for water. It forms with barytes a soluble salt, and it gives with corrosive sublimate a red precipitate soluble in an excess of acid. When we pour into it some drops of oxymuriatic acid, the new substance is instantly regenerated: when heated with black oxide of manganese, the red and brown oxides of lead, iode is disengaged, and the oxides are reduced into the state in which they are generally soluble in the acids. The red oxide of mercury makes the acid pass to iode, and we may conclude that all the oxides which cause the muriatic acid to pass to that of oxymuriatic will also pass a portion of the new acid to the state of iode. Finally, this acid dissolved in water, and subjected to the action of the Voltaic pile, appears at the positive pole in the state of iode. When once engaged in a combination, it is not easy to separate it. The sulphuric acid, for example, when placed in contact with the combination of the new acid, and with potash, gives sulphurous acid, and the new substance is liberated: the nitric gives nitrous acid. If we employ the phosphoric and boric acids, dry or dissolved in water, they produce no decomposition.

It is now easy to conceive what happens when iode is placed in contact with other bodies.

With hydrogen at a high or low temperature we obtain the new acid; but it is not usually pure, because it has the property of dissolving a great quantity of iode, which resists the action of hydrogen.

Sulphuretted hydrogen speedily takes the colour from iode, and makes it pass to the state of acid, depositing abundance of sulphur: it also produces the same effect when the new substance is in combination with the alkalis, forming brown or colourless solutions. It is to be remarked, that when we precipitate by the sulphuretted hydrogen gas a solution of iode in ether or alcohol, sulphur is not deposited in any sensible quantity.

The sulphurous acid speedily converts iode into acid, passing itself to the state of sulphuric acid *. The phosphorous acid and the sulphuretted sulphites also give existence to a new acid.

* This assertion contradicts a fact stated in the next paragraph. Possibly the sulphurous and sulphuric acid should be transposed. Edit.

Hence

Hence we may conclude, that in the sodas of sea-weed in which there is abundance of sulphuretted sulphites, the new substance is in the state of acid; it is not even manifested in the mother waters of these sodas, except when the sulphuretted sulphites are destroyed.

Iode is not altered by charcoal and sulphurous acid, because these substances cannot furnish it with hydrogen in order to pass to the state of acid. It does not decompose water at a low or a high temperature. It takes the colour from indigo, and is driven from its combinations by the mineral acids, and even by the acetous acid. It combines with most of the metals without any gas being extricated. When any of these combinations are made under water, for example, that with zinc, nothing is extricated; the liquor, at first strongly coloured, soon becomes as limpid as water: the alkalies precipitate from it a substance which has all the characters of oxide of zinc, but which nevertheless retains a little of the new acid. Water has also been decomposed, and oxide of zinc and the new acid are produced. This combination, like all those which contain the new acid, gives sulphurous acid when we treat it with sulphuric acid. Eighteen grammes of iode dissolve nearly three grammes and a half of zinc: hence we may conclude that the relative weight of oxygen is as 1 to 20, or 15 to 100. With the oxymuriatic acid a compound is formed of an orange yellow colour, crystalline, volatile, deliquescent, and appearing to exist in two different proportions.

Iode forms, it is said, a fulminating powder with ammonia; but the theory of this phænomenon is very simple, when we consider that the iode has a great tendency to combine with hydrogen.

From the above description we cannot avoid comparing iode with chlorine, and the new acid with the muriatic acid. It is also equally remarkable, that the hydrogen is constantly necessary to make the iode pass to the state of acid. It appears that this substance performs in nature the same functions with respect to one class of bodies, that oxygen performs for another. All the above phænomena may be accounted for by supposing that the iode is an element, and that it forms an acid on being combined with hydrogen; or rather, that this last acid is a compound of water with an unknown base, and that iode is this same base united with oxygen. The former hypothesis appears more probable than the latter, and it serves at the same time to give more probability to that in which we consider the oxymuriatic acid as a simple body. On adopting the new acid, the name which suits it best will be that of the *hydriodic acid*.

XIV. *On*

XIV. *On the Discoloration of Silver by a hard-boiled Egg.* By *Mr.* J. Murray, *Lecturer on Experimental Philosophy.*

London, 21st January, 1814.

Sirs, The discoloration of silver by a hard-boiled egg is a question difficult of solution. It may appear to be an anomalous phænomenon.

Observation and experiment enable me to state the following circumstances:

If a slip of polished silver permeates the *albumen* of the egg *exclusively*, it suffers *no change*; but if it also penetrates the *yolk*, it is instantly *tarnished*, and (what is worthy of particular remark) *the stratum* of the metal in *contact* with the *albumen* alone is *coloured*, while the *surface* of the silver embraced by the *yolk retains* its *pristine lustre* and *character*. The effects are more imposing, if the silver be suffered to pass through the first stratum of albumen, then through the yolk, and then again through the albumen. The impression is also more sensible the harder the egg.

There are two views in which the phænomenon may be considered; 1st, an electrical phænomenon; 2dly, a chemical effect: —if the former, which I conceive it to be, then it is a modification of that Voltaic circle which Davy announces, and where *one metal* is concerned. In the present case, however, there is only *one* menstruum, namely the *yolk* furnishing *sulphuretted hydrogen*. The effect produced at each end of the silver, it follows from hence, must be a *positive* phænomenon. The albumen is decomposed, and the olive oxide of silver produced, compounded according to Davy of 100 parts silver and 7·3 parts oxygen.

Were it the sulphuret of silver, it ought to be tarnished *only* on that part of the slip which the yolk envelops. It receives the oxygen, according to my estimate of the phænomenon, from the albumen, which is known to contain it.

"Si quid novisti rectius istis,
Candidus imperti."

I may again revert to the subject.

I observe that Mr. Walker of Lynn has, in No. 187, page 368, of The Philosophical Magazine, given a theory of combustion as deduced from Galvanic phænomena. In my lecture on Galvanism at Lynn, I took occasion to point out that there were two opinions respecting the evolution of oxygen and hydrogen at the respective poles of the Voltaic circle: 1st, Resting on the supposition that water is composed of hydrogen and oxygen, that a decomposition might take place, and these constituents be liberated

rated from their combination in the gaseous form. 2dly, Considering water as an element, an undecompounded body, and that the *radicles* of oxygenous and hydrogenous gas were communicated by the positive and negative wires, and these combining with the water, probably in definite portions, constituted gaseous oxygen and hydrogen. This was the opinion of Ritter of Jena, and I remarked that it was in harmony with our best views of electrical phænomena. How can we account for the electrical spark not only decomposing but recomposing water, on the latter supposition?—two *opposite* effects resulting from the *same cause?—analysis* and *synthesis* accomplished by *similar* means? —*destruction* and *reunion*, by *one agent*, under *identical* modifications? It is hard to be conceived. But a more satisfactory solution is obtained, if we admit that oxygen and hydrogen in the form of gas are elements (evolved by means of attrition in the electrical machine, or the action of acid and alkaline menstrua on metallic bodies in the Voltaic trough,) united with water: then, these gases in definite proportions, by passing the spark through them, may be united, and the radicles form a neutral effect;—when the power of holding the water in solution ceasing, the moisture is precipitated.

There is much obscurity in Mr. Walker's solution of the phænomena of combustion, arising from his using indiscriminately the terms *heat* (caloric) and *combustion*. Now caloric (the *matter of heat*) and *combustion* (the *act of ignition*) are *not identical*. What may be collected, however, from the general tenor of that paper, is merely the *theory of Lavoisier in a new dress*. Mr. Walker may please to be informed that combustion takes place where there is not the least evidence of the existence of oxygen, as that elicited from the union of sulphur and some of the metals, &c.

I solicit your pardon for this digression, and am with much respect,

Sirs, your humble servant,

JOHN MURRAY.

To Messrs. Nicholson and Tilloch.

XV. *A Mathematical Question. By Sir* H. C. ENGLEFIELD, *Bart., F.R.S. F.A.S., &c. &c.*

Petersham, Jan. 18, 1814.

SIRS, THE very extraordinary powers of the American boy [Zerah Colburn] in finding the roots of numbers, among other wonderful effects of mental computation, led me lately to consider the subject; and in the course of what I may call a mechanical dissection of numbers, I fell on the method which, if you do

do not think it beneath the notice of your Journal, I will propose to the sagacity of your readers to discover. I know that it is rather out of fashion to offer these sort of questions to the public, though it was very common in the beginning of the last century, and was, I believe, attended with much advantage to science, by producing a widely extended emulation: even the questions yearly proposed in The Ladies' Diary are, I am persuaded, very advantageous to the studious in mathematics.

The question I propose is this:

Of any cube number under a million give the figure of the unit, the two last figures, and the number of places, instantly and without any aid of writing to name its cube root. For example, let the cube 438976 be the number whose root is to be named. Given, the two first figures 43, the last figure 6, the number of places six. The root is to be immediately named.

Should the ingenuity of some of your correspondents not find out the very simple method of doing this, I will with great pleasure communicate it to you for insertion when required.

I do not pretend that there is much use in this, but it may be an object of curiosity to some of your numerous readers.

I am, sirs, your obedient servant,

H. C. Englefield.

To Messrs. Nicholson and Tilloch.

XVI. *On the Quantities of Heat developed in the Condensation of the Vapour of Water, and in that of Alcohol.* By Benjamin *Count* Rumford*.

§ 1. *Of the Quantity of Heat developed in the Condensation of the Vapour of Water.*

Having filled the calorimeter and placed it on its stand, a current of vapour was introduced into the serpentine through a cork placed in the lower aperture of the serpentine. This cork having been perforated with a hole two lines in diameter, in the direction of its axis, a small cork (two lines in diameter and two in height) was fitted into it, and four other holes about a line in diameter, pierced horizontally through the sides of the large cork at two lines below its upper extremity, and communicating with the hole two lines in diameter in the axis of this cork, afforded a passage to the vapour, to admit of its entering by four small channels horizontally into the serpentine.

As the apertures of these small channels were higher than the level of the flat bottom of the serpentine, the water which re-

* This paper was read before the French Institute as a supplement to the Count's Inquiry into the Heat developed by Combustion. Vide Phil. Mag. vol. xlii. p. 236.

sulted

sulted from the condensation of this vapour, did not prevent the vapour from continuing to flow through these passages.

This vapour came from a long-necked matrass containing distilled water, which was put on a portable stove placed in a chimney at some distance from the calorimeter; and in order to stop all direct communication of heat between the stove and the calorimeter, the former was masked by plates, and the tube which conducted the vapour to the calorimeter was well covered with flannel.

The cold water which filled the calorimeter was of a lower temperature than that of the chamber by 6° of Fahrenheit and when the thermometer of the calorimeter announced an augmentation of temperature by 12° of Fahrenheit, an end was put to the experiment.

The water produced by the condensation of the vapour in the serpentine was carefully weighed; and from its quantity, as well as from the heat communicated to the calorimeter, the heat developed by the vapour in its condensation was determined.

As a small part of the heat communicated to the calorimeter proceeded from the cooling of the water condensed in the serpentine, after the vapour had been changed into water, an account was kept of this heat. It was supposed that the water at the moment of condensation was at the temperature of 212° F. being that of boiling water; and it was determined by calculation, what part of the heat communicated to the calorimeter must have been owing to this boiling water.

In making this calculation, no account was taken of the difference in the capacity of water for heat, which depends on its temperature: this is but imperfectly known; and besides, the correction which would have been the result, could not but have been very small.

The following are the details and results of two experiments made on the 21st of January 1812.

Number of Experiments.	Temperature of the Room.	State of the Calorimeter (equal in capacity for Heat to 2781 Grammes of Water.)			Quantity of Vapour condensed into Water in the Serpentine.	Result.
		Temperature at the Beginning of the Experiment.	Temperature at the End of the Experiment.	Elevation of its Temperature.		Quantity of Water which may be heated 1° F. with the Heat developed in the Condensation of 1 lb. of Vapour.
					grammes.	*lbs.*
1	61°	55°	$67\frac{1}{2}$	$12\frac{1}{2}$	29·61	1029·3
2	$62\frac{1}{4}$°	$57\frac{1}{4}$°	$67\frac{1}{2}$	$10\frac{1}{4}$	24·4	1052·3
					Mean Result	1040·8

By

By expressing the mean result of these two experiments in the way employed by Mr. Watt and others, I shall say that 1040 degrees of heat (Fahrenheit) are liberated in the condensation of steam, and that consequently this very quantity of heat is employed and rendered latent when the water, already at the temperature of boiling water, is changed into steam.

The duration of each of these two experiments was from ten to eleven minutes, and I had boiled the water some time in the matrass (to drive out the air which it contained) before I directed the steam from it into the serpentine of the calorimeter.

As the results of these experiments have been very uniform, and as they agree very well with the latter experiments made by Mr. Watt with a view to determine the same question, I have not thought it necessary to repeat them.

I have besides been very much occupied with the following branch of my inquiries.

§ II. *Of the Quantity of Heat developed in the Condensation of the Vapour of Alcohol.*

As chemists are not agreed as to the state of the elements of the water which exist in alcohol, I thought that, by determining with precision the quantity of heat which is developed, we should be better able to form conjectures as to the state of the water, if it be at all times found in this inflammable liquid.

The results of the experiments which I made with alcohol are less regular than those of the experiments made with water, as might have been expected; but they have nevertheless been sufficiently uniform to establish a fact, which will be regarded without doubt as very curious and important.

As the vapour which is extricated from spirit of wine when boiled, varies a little with the intensity of the fire used in boiling it, I took care to note the time which was taken in every experiment, in order to be able to judge, by comparing the quantity of vapour condensed, with the time employed to form it, of the intensity of the heat employed to boil the liquid.

In the following table we shall see the details and results of five experiments made on the same day (January 21, 1812) with alcohol of different degrees of strength. The capacity of the calorimeter was always equal to that of 2781 grammes of water, and the thermometer employed was that of Fahrenheit.

TABLE.

Number of Experiments.	Specific Gravity of the Alcohol employed.	Time employed in the Experiment.	Temperature of the Apartment.	State of the Calorimeter.			Quantity of Alcohol condensed in the Calorimeter.	Result.
				Temperature at the Beginning.	Temperature at the End.	Elevation of its Temperature.		Quantity of Water which may be heated 1° of F. with the Heat developed in the Condensation of one Pound of Vapour.
			min				*gram.*	
1	85342	7	61°	54¼°	68½°	14¼°	69·86	499 54
2	85342	5	61°	56°	66¼°	10¼°	52·21	476 83
3	84714	8	60½°	55½°	65½°	10°	48·82	500·03
4	81763	4¼	61°	56°	66½°	10½°	56·61	479·92
5	85342	6½	64°	57°	71½°	14½°	71·31	499·65

On determining, by calculation, the quantity of water which may be heated *one degree*, by the heat developed in the combustion of one pound of this vapour, I took care to keep an account of the difference between the capacity of water for heat and that of alcohol, when I determined how much heat should have been communicated to the calorimeter by the alcohol, and produced by the condensation of the steam, by being cooled in the worm.

In order to prove the state of the elements of the water which exist in the steam of alcohol, it must be shown how much water these elements ought to form.

We shall select the experiment which was made with alcohol of the specific gravity of 81763, and which contained the least water. The quantity of steam condensed in this experiment was 56·61 grammes.

In 100 parts of this alcohol there were
91·79 parts of pure alcohol of Lowitz, and
8·21 parts of water.

Consequently there were in the 56·61 grammes of alcohol condensed in the calorimeter,
51·962 grammes of alcohol of Lowitz, and
4·648 of water.

Now, as M. de Saussure has shown that there are 47 parts of water in 100 parts of alcohol of Lowitz, there must have been 24·422 grammes of water in the 51·962 grammes of alcohol of Lowitz, which were condensed in the calorimeter.

 If

If to this quantity of water (=24·422 grammes) we add the 4·648 grammes which were found mixed with 51·962 grammes of alcohol of Lowitz, in order to compose the 56·61 grammes of alcohol employed in the experiment, we shall have 29·07 grammes of water which ought to have existed, ready formed either in the common state of water, or in some other state, in the 56·61 grammes of alcohol condensed in the calorimeter.

But the condensation of 29·07 grammes of steam into liquid water ought to have of themselves furnished more heat than we had in the experiment in question, in the condensation of these 29·07 grammes of elements of water, with 27·57 grammes of carbon and hydrogen, which concur, with these elements, in forming the steam of the alcohol which was condensed.

If we apply a similar calculation to the results of the experiments made with alcohol which contained more water, the result of the inquiry will be still more striking.

In the experiment No. 5. the alcohol employed was of the specific gravity of 85324: consequently 100 parts of this alcohol were composed of 77·88 parts of alcohol of Lowitz, and 22·12 water.

And in the experiment 71·31 grammes of vapour of alcohol were condensed.

There were therefore in these 71·31 grammes of condensed alcohol,

55·688 grammes of alcohol of Lowitz, and
15·622 grammes of water.

In the 55·688 grammes of alcohol of Lowitz there were 26·102 grammes of water, according to the analysis of M. de Saussure; and this last quantity of water (=26·012 grammes) added to the quantity found above, viz. 15·622 grammes, makes 41·727 grammes of water which ought to have existed, either as steam or otherwise, in the 71·31 grammes of alcoholic vapour condensed in the calorimeter, in the experiment in question.

In order to simplify our calculation, and to render our comparisons more striking, we shall show how much pure water, in vapour, ought to have been sufficient to furnish, in its condensation, the same quantity of heat which was furnished by the condensation of 71·31 grammes of alcoholic vapour, in the experiment in question.

In this experiment the temperature of the calorimeter was raised to $14\frac{1}{2}$ degrees of Fahrenheit.

In the second experiment, made with the steam of pure water, the temperature of the same calorimeter was raised $10\frac{1}{2}$ degrees of Fahrenheit, with the heat developed in the condensation of 24·4 grammes of this vapour.

Conse-

Consequently the temperature of the calorimeter must have been elevated to $14\frac{1}{2}$ degrees of Fahrenheit, with the heat which must have been developed in the condensation of 33·695 grammes of steam from pure water.

Now as the hydrogen and the oxygen forming the elements of 41·727 grammes of water, which are found to form constituent parts of the 71·31 grammes of vapour of alcohol condensed in the experiment in question, only furnished in their condensation the same quantity of heat as 33·695 grammes of steam of pure water should have furnished, it is clearly proved, in my opinion, that these elements are not so united as to form water, so long as they concur in the formation of alcohol.

I have discovered that the vapour of sulphuric ether furnishes about one half less of heat in its condensation than that of alcohol, and consequently one fourth only of what is furnished by the steam of water of equal weight; but having been interrupted by an accident in the course of my experiments with ether, I am desirous of finishing them before I publish the results.*

XVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 20. THE President in the Chair. Sir H. Davy communicated, in a letter to the President, a long paper from Paris, on a new gas discovered in that city by M. Courtois, a manufacturer of saltpetre. It appears that this gas was discovered above two years ago; but such is the deplorable state of scientific men in France, that no account of it was published till the arrival of our English philosopher there. M. Courtois communicated his discovery to Clement and Desormes, who made some experiments with the gas, and latterly M. Gay Lusac has devoted his attention to an examination of its history and properties. Mean time Sir Humphry has made a great number of experiments on it, and would have made several more had he not wanted the necessary apparatus in Paris. M. Courtois was led to the discovery by observing how rapidly his metal pots were corroded in preparing the different kinds of sea-weed, which he used for making carbonate of soda. When the soda is extracted from the sea-weed, the new gas is easily disengaged; by pouring strong sulphuric acid on the residuum, at

* In a subsequent number we shall give the learned author's "Inquiries into the Capacity for Heat, or the calorific Power of various Liquids," as being connected with the foregoing memoir.—EDITORS.

the temperature of 158°, a beautiful dense violet-coloured elastic fluid rises. This the French chemists propose calling *iode* gas, from ἰώδης, *violaceous*; but Sir Humphry, considering the English idiom, denominates it *violaceous* gas. Its properties are singular: combined with hydrogen, with phosphorus, and with oxymuriate of silver, (argentane of Davy) it forms a peculiar acid; it is a simple or uncompounded gas, at a suitable temperature a permanently elastic fluid, but heavier than any known gas, 100 cubic inches weighing 95·5 grains: it is a non-conductor of electricity, experiences no change exposed to the action of the Voltaic battery with charcoal, is not inflammable, and does not support combustion. As a simple substance it has many analogies with oxygen, chlorine, and the alkalies: like oxygen, it rapidly unites with the metals: mercury, tin, lead, zinc, and iron, are converted by it, in a moderate temperature, into salts of orange, yellow, and brown tints, which are soluble in spirits, ether, and water, and form beautiful pigments, and most probably may be equally serviceable in the dye-house. Exposed to a moderate cold it condenses into solid plumbago-coloured crystals. Combined with hydrogen, it forms what the French call hydroionic gas. Like the alkalies, it unites with oxygen, from which it can be expelled by heat. The existence of this substance confirms the opinion previously given by Sir Humphry, that acidity and alkalescence do not depend on any specific principle, but on certain modifications of matter. This chemist concludes his important paper with some observations on the necessity of a new nomenclature, and proposing several arbitrary terminations to distinguish the various substances which, according to the principles of a significant nomenclature, would be called *iodais*. But his observations on this head are, as usual, submitted with the utmost diffidence, and merely as hints on which some general principles may be founded. It is, indeed, evident that the whole nomenclatural theory of Lavoisier is completely set aside by the subsequent discovery of facts, and it is time that chemists would unite together to form a new chemical vocabulary better adapted to the actual state of our knowledge.

XVIII. *Intelligence and Miscellaneous Articles.*

METEOROLOGICAL QUERIES.

FROM reading Adams's extracts from his Journals of the weather, I am induced through your valuable miscellany to put two questions;

Has

Has the moon a particular influence on the seasons?

Has *not* the moon a particular influence on the weather?

The first question arises from a note in the work in page 2, alluding to the year 1811. The second question arises from the observations whereby to foretel the weather, in page 93.

I wish, as my Lord Gray and the Duke of Gordon observe, that these journals were more frequently sent from the silent closet of the meteorologist before the eye of an enlightened public.

S. H.

ERUPTION OF VESUVIUS.

Naples, Dec. 26.

Yesterday, at five in the evening, commenced one of the most formidable eruptions of which there is any record in the history of Vesuvius. Happily, though it presented a terrific spectacle, it caused no great damage. The explosion began by a shower of volcanic gravel, which was followed by a violent eruption of lava. This ignited matter having divided itself into two torrents, flowed over the ancient lavas towards the Torre del Greco. At ten in the evening the first torrent stopped; but the second continued to proceed towards Bosco-Reale and Bosco-Tre-Case.

This morning the apparent calm of the volcano was followed by an explosion resembling a violent discharge of cannon. A column of volcanic ashes rose in the air, and obscured the horizon. At the moment we are writing the eruptions still continue. Redoubled shocks make the houses of the city shake, but have as yet done no damage. We do not know the direction of the lava.

The Government has taken every necessary measure to afford assistance to those who may have need of it. Yesterday the King sent one of his equerries to inform himself of the state of things, and to-day the monarch came out himself to observe the eruption. When arrived in the neighbourhood of the fiery torrent, he spoke to every person with that affability which characterizes him, and pointed out to those who appeared terrified, that the direction of the lava secured us from any danger.

P.S. It is two o'clock; the eruption appears to have ceased.

On the 29th of December a second explosion of the Felling Colliery took place, causing destruction to nearly every living creature within the range of its explosion. Nine men, 13 boys, and 12 horses, fell victims to the fury of the blast, and 8 more pitmen were severely scorched, though likely to recover. By this heart-rending occurrence, 8 widows, and 18 fatherless children, are becoming the deserving objects of public benevolence*.

* Why has no attempt been made to prevent these dreadful calamities, by applying to coal-mines Mr. John Taylor's cheap and simple exhausting machine, described in vol. xxxviii. p. 1?0, which has been found so effectual in the mines of Devon?

A contagious fever producing an alarming and daily increasing mortality having prevailed at Gibraltar during the summer and autumnal months of 1813, the following account of it, drawn up by a medical officer of that garrison, will be found interesting. After stating that on the 3d of December last the disease totally disappeared, the writer proceeds:

When the disease commenced, the population was 15,600 inhabitants; and the garrison with their families amounted to 5,500. Of the inhabitants, nearly one-half took shelter on board of ships, or were forced to encamp on the neutral ground, where they all continued well, with very few exceptions, none having been taken ill after they had quitted the place six days. Of the inhabitants who remained, to the number of 7,870, upwards of 3,800 had had the disease in 1804, who all escaped, no well-authenticated case having appeared of any person taking the fever a second time: of the remainder, not more than 40 escaped an attack of the fever; yet upwards of 2,600 of the garrison and their families escaped, by their being encamped outside of the town and on the heights above it, and avoiding all communication with the town.

The disease appeared to originate solely from contagion, as every person outside of the walls, or who kept themselves in complete seclusion, remained in perfect safety; and all the numerous vessels lying in the Mole and Bay, though crowded with inhabitants, continued perfectly healthy whilst they avoided communication with the town; but in six different instances, where they neglected this precaution, the fever appeared on board.

The average mortality of deaths was one in five. In more than half of the fatal cases, the black vomit took place. Yellowness of skin was rather an uncommon symptom, and seldom occurred but in fatal cases, and then was of a dingey mahogany colour, and commenced a few hours before death.

The faculty now seem generally to agree that the disease is the same as that of 1804, and that it is a distinct disease from the bilious remittent fever, and has been introduced here by imported contagion, and propagated by contagion alone, as it has been one of the coolest and healthiest seasons ever known.

Some opulent planters in Georgia (says an American paper) are turning their attention to the cultivation of the sugar-cane. From experiments lately made, it is ascertained that an acre planted with cane will yield sufficient to fetch 2400 dollars, deducting the expense of cultivation, which is about four hundred dollars.

The

The French Gazette of Health for November contains a detailed relation of the sufferings of a young girl of eight years old, into whose ear a spider had crawled. She experienced momentarily a nervous fit resembling epilepsy, of which the strength gradually increased, impressing those around her with a fear that it would speedily become fatal. The surgeon, unable to extract the spider, poured oil of olives into the ear; hereupon she experienced some convulsive movements, longer and more violent than those she had before; and when they terminated she was quite relieved.

Professor Mangeli has published in the Milan Journal, a long report upon the action of the venom of vipers. He states, as the result of his experience, that ammoniac is the only sovereign remedy for the bite of those reptiles, and that opium and musk, which have been hitherto prescribed by Italian physicians, have no certain effect.

An Havanna paper of the 7th inst. mentions that an aged priest, in Guatimala, had lately applied himself to the production of opium in that province, and had succeeded to a degree that promised to make his discoveries a great national benefit. The Guatimala opium was said to be of a much superior quality to that obtained from the Levant.

Cotton and indigo are said to have been successfully cultivated in the principality of Cintra, in Calabria, Nola, and other places in Italy. From the leaves of the latter plant, indigo of a quality not inferior to the American is asserted to have been extracted.

Dr. Adams has in the press his long projected work on the erroneous opinions and consequent terrors usually entertained concerning hereditary diseases. Connected with the subject are some remarks on the attempts at reducing cutaneous complaints to orders and classes, and the unnecessary revival of so great a number of exploded Greek terms.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Pope, of the city of Bristol, perfumer, for an instrument or instruments, to be used jointly or separately, for ascertaining a ship's way at sea, and assisting in determining the longitude.—16th Nov. 1813.—6 months.

To William Burge, of the city of Bristol, confectioner, for certain improvements in the construction of fire-places.—16th Nov.—2 months.

To

To James Brumsall, of Plymouth, in the county of Devon, tailor, for certain improvements in different stages of rope-making, and in machinery adapted for such improvements.—16th Nov. —6 months.

To Edward Charles Howard, of Westbourn Green, in the county of Middlesex, Esq. for certain improvements in the process of preparing and refining sugars.—20th Nov.—6 months.

To Frederick Cherry, of Croydon, in the county of Surry, veterinary surgeon in the army, for certain improvements in the construction of various articles of an officer's field equipage.—23d Nov.—2 months.

To Jeremiah Donovan, of Craven Street, Strand, in the county of Middlesex, Esq. and John Church, of Chelsea, in the same county, soap-boiler, for their discovered improvement of saponaeous compounds for deterging in sea water, in hard water, and in soft water.—23d Nov.—6 months.

To Richard Mackenzie Bacon, of the city of Norwich, printer, and Bryan Donkin, of Fort Place, Bermondsey, in the county of Surry, engineer, for their improvements in the implements or apparatus employed in printing, whether from types, from blocks, or from plates.—23d Nov.—6 months.

To James Bodmer, of Stoke Newington, in the county of Middlesex, gentleman, for his method of loading fire-arms, cannon, and all ordnance, except mortars, at the breech, with a rifle or plain bore; and also a touch-hole for fire-arms and ordnance, and also a moveable sight for fire-arms and ordnance.

To Edward Biggs, of Birmingham, in the county of Warwick, brass-founder, for his method of working stamps by a steam engine, water, or horse power.—23d Nov.—2 months.

To John Duncombe, of Woolwich, in the county of Kent, civil engineer, for his improvement to mathematical and astronomical instruments in order to render them more portable, accurate, easy, expeditious and certain in their application to topographical and nautical surveying, the mensuration of terrestrial and celestial angles, and the direct distances of inaccessible objects, at one station, by land or sea, without the usual modes of calculation, by a new index which ascertains the measured quantity of an angle to any proposed rational degree of precision, by rendering the division of the minute parts hitherto imperceptible to the senses, truly conspicuous and distinctly legible by the common naked eye; also, by an attached new parallel movement, the natural sine and co-sine of such angles are precisely obtained to any eligible radius, without tabular or other reference; and by a detached similar movement, the direct distance of an inaccessible object is accurately measured at one station, without trigonometrical or other calculation; and a new improved compass, whose index points

points due north and south, and which is capable of adjustment according to the known or observed variation of the magnetic needle.—25th Nov.—2 months.

To John Cragg, of Liverpool, in the county palatine of Lancaster, Esq. for certain improvements in the facing of exterior and interior walls of Gothic or other structures, built of brick or other material, with strong milled or sawn slates bound and secured by mouldings, grooves and ties of cast iron, in such a manner as to have the appearance, when sanded, of finely wrought stone-work in ornamental pannels or otherwise, with ceilings of correspondent tracery form and character of the same materials, which may be supported by pointed arches rising from single or clustered columns of cast iron or otherwise; and in capping buttresses in Gothic architecture with highly enriched pinnacles or finials of cast iron only; the which being connected by metal, with the spouts also of metal, and carried down to the ground from conductors for the protection of lofty buildings from the effect of lightning; also for a spiral stain (wholly of cast iron) of a light and simple construction, which may be carried up or inserted within the corner of a buttressed tower wall, or in the cylinder of a small turret; by which mode of facing, adorning, and constructing the said several parts, churches or other buildings of pure Gothic design may be erected of brick, and finished with light ornamental carved-work of appropriate taste and elegance, at less expense than if wrought in stone, and in materials that will endure.—29th Nov.—2 months.

To Maurice De Jough, of Kentish Town, in the county of Middlesex, for his improvements in the method or methods of manufacturing or preparing madder roots and madder.—29th Nov.—2 months.

To Isaac Willson, of the city of Bath, gentleman, for certain improvements upon stove-grates to prevent smoky rooms, and for obtaining an increased heat from the same quantity of fuel.—29th Nov.—6 months.

To Samuel Tyrrell, of Peddinghoe, in the county of Sussex, farmer, for his broad-cast sowing machine.— 4th Dec. —2 months.

To John Bateman, of the township of Wyke, in the county of York, for his improvement on musical instruments.—9th Dec.—6 months.

To Thomas Wright, of Great St. Helen's, in the city of London, broker, for his improved method of making a composition or mixture for dyeing scarlet and other colours.—9th Dec.—6 months.

To John Swarbreck Rogers, of the city of Chester, merchant, for his mode of spinning or making a species of wool into yarn, either

either by itself, or with any other material with which yarn may be beneficially used in various branches of manufacture.—14th Dec.—2 months.

To Joseph White, of Leeds, in the county of York, millwright, for his improvements in steam engines.—14th Dec.—6 months.

To William Allamus Day, of Poplar, in the county of Middlesex, for his method of extracting all the gross or mucilaginous matter from Finks or Greenland blubber produced from whales when boiled into oil; which method not only renders the oil so boiled more free from its usual rancid smell and taste, but in a great degree adds to its burning and inflammable qualities.—20th Dec.—2 months.

METEOROLOGY.

During the present month the fall of snow has been greater than for many years, and the thermometer has been lower generally throughout the kingdom than has ever been known.

At Basingstoke, Hampshire, at 7 a. m. on the 10th Fahrenheit's thermometer was at −3°; at 10 a. m. at 8°+; at 7 p. m. at 3°+.

January 9,	in the morning at Edinburgh . .	24°+
15,	at Kelso	10°
17,	at Glasgow, at 8.30 a. m. . .	7°
17,	in the fields near Glasgow . .	5°−

The following observations on Fahrenheit's Thermometer were made at Croft House, near Bradford, at a little before nine o'clock, on the mornings of each of the following days:

January 4	12°+	January 14	15°+
7	17°	15	22°
9	18°	16	22°
10	10°	17	3°
11	21°	18	18°
12	22°	20	28°

The Register Thermometer at Jones's, in Oxendon-street, on the night between the 9th and 10th, was at 7 degrees Fahrenheit; on the same night, at Petersham, it was at 2 degrees, which is the greatest cold we have had since between the 24th and 25th of December 1798, when the Register in Tilney-street was at zero; Sir George Shuckburgh's, in Park-street, 2 degrees below zero; Mr. Cavendish's, at Clapham, 7 below—and near Maidstone, 11 or 12 below zero, which was the greatest cold ever observed in England.

Meteoro-

Meteorological Observations made at Clapton in Hackney, from December 17, 1813, *to January* 19, 1814.

Dec. 17–20, 1813.—The weather clouded for the most part, the Thermometer generally above 40° in the day, with damp hazy atmosphere and southwesterly winds. On the night of the 20th a frost set in, with Thermometer 24°.

Dec. 21–25.—Damp atmosphere, a great deal of cloud, and some rain during this period.

Dec. 26.—Clear day, with tufts of *cirrus* and cooler air. Wind northerly.

Dec. 27.—A very great change took place to-day; a thick white *stratus* or fog, of such density as to prevent carriages from finding their way, prevailed all day, and for a long period afterwards.

Dec. 28.—Fog somewhat less, but very great.

Dec. 29.—Fog denser in London, but rather less in the country.

Dec. 30.—Fog so far cleared off in the country as to let the sun be seen through it, but very dense in London.

Dec. 31.—Clearer in the middle of the day over-head, and *cirrus* observed in tufts; dense fog at night. Barometer 30.36. Therm. at night 21°.

Jan. 1, 1814.—Fog cleared off again a little in the day-time; was exceedingly dense and dark in London. Therm. at night 18°. Bar. 30.10.

Jan. 2.—Fog less; clouds seen aloft. Barom. sinking, 29.85. Therm. 30°.

Jan. 3*.—A thaw seemed coming with clouded sky, but the frost and fog returned at night. Barom. got to 29.61.

Jan. 4.—Snow commenced to-day from NE. Barom. 29.35. Therm. 25.

Jan. 5.—Snowing all day, and it lay very thick. Windy night from W. Barom. 29.15.

Jan. 6†.—Windy. Snow lies deep. Barom. 29.20.

Jan. 7.—Cold and clouded early; then *cirrus* and *cumulus*. Barom. 29.68. Therm. at night 12°.

* This remarkable fog, of so long continuance, appears by accounts to have commenced earlier in the southern and western parts of England than in the northern and eastern. It was very partial at times, and often suddenly abated and returned. To speak of its nature in meteorological language, I should say it was a highly electrified *stratus*, and was accompanied the whole time by a gradually falling Barometer and an increasing frost.

† The Barometer began rising to-day, and continued to rise till the 11th; during all the interval a very hard frost prevailed. On the 11th the Barometer began to sink again.

Jan.

Jan. 8.—*Cirri* and other doubtful clouds; *stratus* partial and temporary. Barom. 29.69. Therm. 22°.

Jan. 9.—Clear aloft, save a few flimsy clouds; intensely cold. The Therm. at night as low as 6°. Barom. 29.78.

Jan. 10.—Very cold; some snow fell from N. in which quarter or NE it has been for the last week. Barom. 29.83. Therm. lowest 19°.

Jan. 11—Snow in small flakes from S. Wind east P.M. Barom. 29.92 in the morning, at night 29.75. Therm. 22°. Gales of wind.

Jan. 12.—Fine cold day; few clouds: cloudy by times at night, and rather windy. N.

Jan. 13.—Clear, and clouds by times. Barom. rising 30.18. Therm. 14°. N.

Jan. 14.—Fair morning; clouds P.M. with gale from S. Barom. 29.84. Therm. 19°.

Jan. 15.—Barom. 29.70. Wind SE. calm. *Cirrostratus*, &c. Sky greenish.

Jan. 16.—Barom. 29.35 rising. Therm. 21° in day. Wind N. Cloudy.

Jan. 17.—Barom. falling 29.42. Clear morning; cloudy, and gale from N. at night.

Jan. 18.—Barom. 29.18 falling. Gale from N. with plenty of small snow.

Jan. 19.—The Barometer to-day was a long time stationary at 29.14; towards night it rose; snow fell from north-east all day with some sleet. The flakes were seldom large. The Thermometer about 30° and 32° all day. Wind in gales and high, and thawing by degrees.

During the previous part of this long and severe frost, there have been very few sea-fowl observed about the marshes, which were formerly so common in such weather; nor have wild geese been observed to go southward. This circumstance suggests the inquiry, what has been the state of the weather in northern parts of our island. Any observations on this subject communicated by northern meteorologists would be interesting. I cannot omit here mentioning another curious circumstance. The greatest haziness and fog known for ages before, is well remembered to have prevailed the year of the last great eruption of Mount Hecla; the late great fog began the day of the present eruption of Mount Vesuvius.

Clapton, Jan. 19, 1814. THOMAS FORSTER.

METEORO-

METEOROLOGICAL TABLE,

Extracted from the Register kept at Kinfauns Castle*, N. Britain. Supposed Lat. 56° 18′.—Above the Sea 90 feet.

1813.	Morning 8 o'clock. *Mean height of* Barom.	Morning 8 o'clock. *Mean height of* Ther.	Evening, 10 o'clock. *Mean height of* Barom.	Evening, 10 o'clock. *Mean height of* Ther.	Depth of Rain. Inch. 100	Nº of Days. Rain or Snow.	Nº of Days. Fair.
January.	30·07	33·00	30·09	33·70	1·05	10	21
February.	29·59	38·00	29·59	38·00	2·66	23	5
March.	30·07	41·90	30·08	40·90	·41	14	17
April.	29·99	43·40	30·04	40·97	·73	11	19
May.	29·84	49·06	29·85	46·93	2·50	22	9
June.	30·10	54·83	30·10	53·00	1·07	9	21
July.	29·89	57·90	29·88	56·50	2·44	13	18
August.	30·09	56·51	30·09	54·16	·63	5	26
September.	30·05	52·40	30·04	52·10	1·72	10	20
October.	29·82	42·30	29·81	43·00	1·70	10	21
November.	29·71	37·50	29·72	37·60	1·47	11	19
December.	29·92	37·51	29·89	37·71	·95	12	19
Average of the year.	29·929	45·375	29·954	44·547	17·33	150	215

* Kinfauns Castle is the residence of Lord Gray.

ANNUAL RESULTS in 1813.

BAROMETER.

Highest	22 Jan.	Wind SW.	30.58	Highest	31 Jan.	Wind NW.	30.57
Lowest	1 Apr.	SE.	28.74	Lowest	17 Feb.	S.	28.73

THERMOMETER.

Highest	12 Aug.	Wind SE.	64°	Highest	30 July	Wind S.	63°
Lowest	26 Jan.	SW.	17°	Lowest	25 Jan.	W.	19°

By one of BARBON's best Double Tube THERMOMETERS.

Highest Observ.	Afternoon of 30 July,	Wind S.	72°
Lowest Observ.	Morning of 26 Jan.	SW.	16°
Mean Temperature for the year 1813,			46.83

Weather.	Days.
Fair	215
Rain or Snow . .	150
	365

Wind.	Times.	
N & NE . .	10	8 o'clock in the Morning.
E & SE. . .	76	
S & SW. . .	101	
W & NW. . .	178	
	365	

RAIN.

		In. 100.
The greatest fall in 24 hours, 16 July,	Wind W.	0·78
The greatest in one month in February	. . . .	2·66
The least . . . in March	. . .	0·41
Total quantity fallen at Kinfauns Castle in 1813	.	17·33

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For January 1814.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Dec.	27	27	30	28	30·52	0	Foggy
	28	27	27	25	·37	0	Foggy
	29	25	28	27	·35	0	Foggy
	30	25	30	28	·36	0	Foggy
	31	27	31	26	·30	0	Foggy
Jan.	1	27	27	27	·13	0	Foggy
	2	28	31	30	29·83	0	Foggy
	3	30	32	28	·68	0	Cloudy
	4	30	31	30	·30	0	Snow
	5	30	32	32	·17	0	Snow
	6	32	32	26	·36	0	Snow
	7	22	27	23	·62	0	Cloudy
	8	23	29	25	·63	0	Cloudy
	9	24	28	18	·65	0	Cloudy
	10	15	28	26	·82	0	Cloudy
	11	22	27	25	·67	0	Cloudy
	12	20	25	20	·50	0	Fair
	13	26	28	20	30·05	0	Fair
	14	20	24	26	29·62	0	Cloudy
	15	26	28	28	·56	0	Cloudy
	16	28	32	29	·32	0	Cloudy
	17	25	29	24	·15	0	Fair
	18	30	32	30	·12	0	Snow
	19	32	32	28	·10	0	Snow
	20	28	30	20	·50	0	Snow
	21	20	26	21	·78	0	Fair
	22	24	28	19	·75	0	Fair
	23	22	31	28	·72	0	Fair
	24	25	27	28	·78	0	Showers of Snow
	25	28	32	27	·88	0	Fair
	26	32	35	35	·45	0	Snow and Rain

N.B. The Barometer's height is taken at one o'clock.

ERRATA.—By an oversight, in putting our last number to press, some typographical errors in Mr. W. JONES's article were left uncorrected.

In page 465, line 1, for "*plane*" read "*plano;*" and in line 31, for "Dr. *Maskeline*" read "Dr. *Maskelyne.*"

XIX. *On the Use of Air-Vessels in Plants.* By Mrs. Ibbetson.

Sirs,—Sir George Staunton observes, "that the leaf of the *Nymphæa Nelumbo,* besides its common use, has from its peculiar structure, growing entirely round the stalk, the advantage of defending the flowers and fruit within its centre from any contact *with the water,* from whatever depth, (unless in case of a sudden inundation,) until it attains the surface." But this property is common to all water plants. There are curious facts appertaining to plants of this kind, well worth showing, since they are phænomena which serve as general rules in nature; and to attain and collect *these,* has, from the first of my dissecting plants, been *my most ardent desire;* especially as (in this case) they are *never,* or *very rarely* varied from. The fact which I now wish to prove, is the use which Nature makes of air-vessels in plants, while I attempt also to explain how air in general is received and placed in them. It was the universal opinion of all the physiologists of the last age, that all plants *have air-vessels:* but dissection has convinced me that *this is a mistake;* that when found, they are too large and beautiful not to be seen and acknowledged: but they are to be discovered in water- and semi-water plants alone. There is indeed a quantity of air mixed in the vessels of all vegetables, and from this circumstance arises that process which accelerates or retards the flow of the sap and other juices: the mixture of air in the sap-vessels is the source of the most wonderful part of the formation of plants in general, since to that is owing the constant fluctuation which the heat or cold produces in the several vessels, filling and emptying them as the thermometer rises or falls: thus, when the cool of the evening, or even a cold blast approaches, the air which nearly fills half the sap-vessels is condensed into a much smaller compass; this causes a *momentary vacuum,* which opens the innumerable leaf-valves of the plant, and in succession those of the hairs, and admits a strêam of rain or dew into the sap-vessels, which then being perfectly replenished, the returning sun as quickly converts into *oxygen* for the restoration of that purity of air so necessary to health; and carbonic acid gas, for the formation of the bark juices, and other combinations. This process takes place in some measure, *in water-* as well as in *land*-plants: but with respect to real *air-vessels, vessels filled with air alone;* they appear, from all the dissections or study I have made on them, to be placed in the plants merely to *support them in the water;* and to be fixed in different directions according to the attitude in which the vegetables are required to stand. When the leaf is to lie on the stream in close contact with it, then the leaf-stem

increases in length with the depth of the water, and it is wholly filled with air-vessels which sustain it in a perpendicular direction to where the leaf begins, and it has a layer of air-vessels also underneath the leaf, to support it on the stream, and keep its upper surface perfectly dry. But when the plant is to be kept above a foot under water, the leaves lose their accompaniment of air-vessels under, and retain only those which surround the leaves, and a bubble at the apex and bottom of the midrib, which is increased or diminished according to the necessities of the plant for rising or falling in the stream. But if leaves are to be maintained in a perpendicular direction in the water, then more art is used for the purpose, which I shall now show in describing the whole formation of a water-lily, or the *Nymphæa lutea;* observing that the dissection is the same in all the species I have been able to procure for the purpose.

That the water-lily has two sorts of leaves, is a fact, I believe, not known even to the worthy Baronet who so well observed them in their native waters *in China:* that they wholly differ in thickness, and in form, as they do also in the various uses for which they are designed, I shall now show. The first I shall describe, is that which swims on the water. A double layer of air-vessels just covered by a skin perfectly impervious to water, forms its lower surface, that which is in contact with the stream; while the upper one has a treble net, instead of pabulum, covered by a double skin through which no water can pass. I have given a sketch of the thickness of the leaf in (Plate II. fig. 1.) that the difference might be well understood, and the use of air-vessels fully exemplified. I have said that the stem lengthened with the depth of the water, but the air-vessels in the midrib stop with the commencement of the leaf, so that it lies quite flat, and the upper surface perfectly dry. But the other leaf is of a very different description: when the corolla has fallen off, and the seeds have dropped from the receptacle, the pericarpium decays, and a new germ, which *is to contain* the next year's fruit and flower, just peeps above the mud, but is wholly covered with water. This is surrounded with leaves of a very peculiar form, flapped into a kind of scollop at the edges; they press the bud in every other part; while the apex of the midrib just keeps *above* the *water*, enabled to do so by a large bubble of air which it encloses, and by all the midrib being full of air-vessels: of course there are none under the leaf, and fewer in the leaf-stalk; but the large opening for air between the skins of the leaf, at the *top scollop*, keeps it constantly upright.

The (fig. 2.) will give an idea of the comparative thickness of the leaf, while (A, fig. 1.) and (B, fig. 2.) will show the shape of the leaf to which each dissection belongs; and its interior formation,

mation, and the different mechanism of the two leaves, will point out the use of air-vessels. The little influence the water has on the essential properties of the plant will be shown, since it in a very trifling degree alters either seed, flower, or fruit. I gave before a letter on Water Plants*. I then showed how the water grass, *Festuca fluitans,* is supported on the stream by the same means *(a layer of air-vessels under the leaf)* and that almost all water plants have *two* sorts of leaves; 1st, floating ones to yield oxygen, and form the different compound juices required by the plant; 2dly, the under leaves to support and invest it, and for many other purposes yet to be discovered; but in the *Potamogetons,* the lower leaves are too narrow to cover the bud, and this is wholly left to the vails. In the *Potamogeton natans,* when the water happens to dry up, the air-vessels by degrees lose all their beauty; they contract in such a manner as soon to become merely common vessels, and then the plant carries the appearance of a *Plantago.* Having found one last summer, I should not have known it to be a *Potamogeton,* but for the observation of Linnæus, who had seen and mentioned the alterations that sometimes took place in the species. This is remarkably the case in many of the semi-water plants, which, instead of decaying for the want of water, become land plants, by losing or contracting their vessels, particularly the *Veronica scutellaria,* and sometimes the *Beccabunga.*

The strange idea that water plants perspired, I think I should not here have shown to be false, as I flatter myself that what I have already in former letters proved, is so plain and evident to common sense, as to carry conviction to every breast that seeks it: but as the mistake originated in *land plants,* by *figures* (which in reality resembled any thing rather than a bubble of water), and as water plants are almost divested of hairs, retorts, or any appearance resembling them, it is proper that I should show what was taken for perspiration in *water plants.* A very light species of *Conferva,* hardly to be distinguished by the naked eye, very frequently covers the floating leaves of water plants, and between its pellucid and slender hairs the water is necessarily detained; the bubbles of oxygen, continually flowing from the leaves, are caught in its diminutive meshes; and the plant, thus situated, appears covered with diamonds, which when swept off by the hand, the no longer imprisoned air *disappears,* but the water (the cause of the phænomenon) still remains *with the Conferva,* as an *apparent proof* of the perspiration; it being supposed to proceed from the bubbles. But I have so often examined the whole matter, so often placed a large magnifier over the leaves

* Philosophical Journal.

and watched them for hours, without seeing *one bubble* of *water transpire*, that I am well assured of the truth of the above fact; and that, when not covered by a *Cryptogamia*, the floating leaves are *always perfectly dry*, which they could not be with the *perspiration attributed to them*. I have said that the water plants have no hairs, that is, that none of the *uninclosed parts* have any; which appears to me to be a strong proof of two facts I have much wished to establish; viz. that the hairs, and those instruments in general taken for perspiration, are on the contrary intended to *bring* moisture to the plants, instead of drawing liquid from them; and that there exists such a thing in vegetation as a skin impervious to water, which covers all leaves and most other parts, and is of peculiar use to water plants. These two points are of great importance to phytology. I have continually brought them forward in every specimen that gave proof of their existence; and former evidence is much added to, I think, by what follows. Though leaves and the parts under water uncovered by vails, have no hairs, yet the flowers which shoot their spikes above, and the buds while thoroughly defended from the stream, *have a few;* and what is most curious, they are filled with water. The bud before the flower develops is covered by a treble vail, so thoroughly impervious to water, that one of them is usually inflated *with air*, that it may more effectually guard the pollen, and prevent the introduction of moisture. Assured that the hairs *were never* replenished from the plant, but that they gained their liquid from the atmosphere alone, I wished to try how hairs when so situated, could obtain the water that inflated them: taking therefore two glasses and placing them one within another, well guarded below from any moisture by a thick luting, I covered the whole apparatus with water, and left it for the night; when the next morning I found the interior of the inward glass strewed with pellicles of water enough to fill all the little hairy cylinders the buds contain; showing that evaporation can pierce through a double glass. There cannot be a stronger proof that the hairs possess some peculiar power which enables them to draw moisture from the atmosphere, than this. Though that I gave in a late letter, shows it is not water alone that is procured in this manner, since *oil* is also *received* by the *hair;* and in roses, the luscious red juice produced or concreted in them, may be traced afterwards entering the plant, and running in appropriate vessels even to the valves of the leaf.

Having now shown all that concerned the leaves and air-vessels of the water plants, I wished to ascertain whether they differed from other plants in this respect; or whether, like them, they formed their seeds and buds in the root? To prove this, I procured a large water-lily of the *lutea* species, and cut many pieces

of

of the root, which I subjected to the microscope in every possible way likely to elucidate the truth. They all presented specimens in which both buds and seeds were plainly to be discovered; one of these, an *horizontal slice*, I shall give a figure of. In the centre of the root, buds will be discovered, and the seeds are imbedded in the alburnum vessel next the bark (fig. 3. C, *buds*,) and (fig. 3. D, *seeds*.) I then divided the stem, and taking a perpendicular piece down the middle, it presented me with the line of life (mounting as it always does in small plants) in the centre, with the buds attached to it; (see fig. 4.) To complete the picture, I continued to cut to the top of the plant, and discovered the buds in a more advanced state, where they are first collected, for the use of communicating to them the different ingredients; their seeds and pollen (fig. 5.) which they had *not yet received*, though so much formed in appearance; for both stamens and seed-vessel *were empty*. I never dissected a plant more fitted to show every truth I wished to prove: 1st, that water interferes less with the essential properties of a plant than could be conceived; that air-vessels are merely supporters; 2dly, that the seed, bud, and pollen, are formed in the root in water plants, and not in the seed-vessel and stamen; 3dly, that there is no perspiration in water- any more than in land-plants, though the latter have few hairs; 4thly, that there is an invisible skin, which covers almost all vegetation, impervious to water; and that the hairs receive their liquid from the atmosphere, and not from the vegetables to which they belong. As I enter into the minutiæ of the formation of plants, these facts become more evident, and not a single vegetable do I dissect without their appearing too positive to be doubted: there is a curious appearance around the flowers of the plants of which I have given a specimen, which I never saw before and cannot account for. (See fig. 3. and fig. 4. at K.) I can at this time procure only the *Potamogetons* and the *Ranunculus aquaticus*, and the flower in them has but just begun to form. That the spiral is discovered in all corollas, is a fact; but to see it all round in this manner is novel, and may, I hope, be the means of procuring further information.

Shall I be pardoned for concluding this letter with breathing a doubt respecting that account which Theophrastus gives, that the *Nymphæa Lotus* sinks under water every night? The very great care Nature takes in all water plants, to defend them from the element till after impregnation, is such, that I cannot believe she would suddenly change her laws, merely to make one flower of a system act in exact contradiction to a known principle. All water plants rise above the stream to perform the impregnation of the seed; and when the flower is under water, before

it blows, it is covered with a treble vail to defend it from the influence of the element; and this defence it loses not till after it has risen above the water. All the species seem admirably to coincide in every particular. We have a native in this country, where the nights are colder in proportion. What reason, then, can possibly justify such a variation? Prosper Alpinus endeavours to prove that the *Nymphæa Lotus* is in every respect the same as a common lily, and adds that they all equally sink at night. Now we know this to be a *mistake*, as not one of the species flowering in England *does so:* but there is another reason that makes me suppose the observation of Theophrastus unfounded; there is *no mechanism* to draw the plant hastily under the water. I have now for some years dissected plants, and I never yet saw a purpose of any kind effected without a very visible means to produce it. A stalk never bends, a leaf turns, or a corolla twists, without the muscles presenting themselves in a proper way to effect the purpose required. If the interior of the stem had been found with spirals sufficient to raise or contract it, I should have credited the account. The *Ruppia maritima*, which draws under the water soon after fructification is over, has a stalk formed with proper mechanism to act thus. In the *Valisneria spiralis* there is also an evident proof of its being made to be drawn under the stream; but *even in these two* plants, they remain above the water till after the fructification is past. That the flower closes at night, and lays its head on the water, is certainly true; but this is also to take care of the stamen and pistil; it is the general watchfulness of Nature to perfect that which is to insure the safety of the future seed, and even the lying down of the flower is to prevent the dew from entering where the petals close, lest it should hurt and explode the pollen too early; for, at a certain time of its formation, it bursts immediately on water getting through the stamen, and becomes (if not ripe enough) quite incapable of performing its part in the fructification of the seeds. This very case seems to contradict the fact advanced by Theophrastus. But may I be so daring as to hint that neither Greeks nor Romans were very methodical in their accounts of the phænomena of nature? And though this *famous Grecian* is thought to be one of the most exact in whatever he records, yet his *flesh-consuming stone*, which he says destroyed in forty days all but the bones of the body laid in it, and turned to stone the *shoes* and every other different material placed in that repository, shows that he believed *that* which he never took the trouble to investigate. As to Pliny, he credited that stones brought forth young; and he repeatedly mentions several sorts of firs that had tap-roots six cubits in length; though there is but one species of

fir

fir that has any tap-root, and that, being the deciduous *Cyprus*, and an American tree, could not be known to him. I merely mention this, not to detract from these great authors, but as lessening the certainty of the evidence when placed in *opposition* to a *real law* of nature, from which we have as yet seen no variation.

Before I close this paper, I cannot but mention a circumstance, though irrelevant to the subject, which proves, I hope, the accuracy of my observations in this respect, and has given me some confidence in them, as agreeing with those of so distinguished a philosopher as Mr. Leslie. I observed in one of my letters last year, that when I placed the spiral wire suddenly in any degree of heat, its motion was all by impulses; whenever I breathed upon it, or exposed it to the focus of the solar microscope, its starts were convulsive: now Mr. Leslie in his description of his new Atmometer, in which he gives so curious an account of the manner in which heat is communicated to water and odours, observes, *that it is by pulsation*. I suppose, therefore, that it is the heat which occasions the motion in the delicate muscles of the spiral wire, and not any action peculiar to itself. I am, sirs,

Your obliged servant,

Cowley Cot, Jan. 14, 1814. AGNES IBBETSON.

P. S. In cutting the larger buds I divided the last cover (fig. 5. E E): it is the one generally inflated with air, and it now looks like a calyx; but it is only a vail to protect it from the water: there were three buds, and they were cut in halves, but I could not show them. At the inside there was such a confusion I could make nothing of it, except that neither pollen nor seeds were yet there. The moment the seeds enter, or the pollen gets into the stamen (the *latter particularly*), the parts inflate, and are much larger at that time than they ever are afterwards. It is impossible, therefore, not to be sensible of the moment in which it is done, *if the flower or bud is divided*; for all is in confusion before, and beautiful regularity succeeds. I shall soon give a view of the buds in every *different stage*, viz. in the *root*, in the *stem*, and just *before blowing*, which will elucidate this subject greatly. But it requires such nice instruments of dissection, particularly very sharp and pointed scissars, that I am waiting for their formation. I have before given the form of the *air-vessels* in fig. 2. Journal 144. There are but three sorts, and it is only the shape that differs; the interior parts and the mechanism are the same in all. (Fig. 6) are the water vessels in the *Menyanthes*. This is the sort Mirbel dissected, I fancy. He says, "Ce ne sont pas des petites utricules comme le disent la pluspart des auteurs, mais *une membrane* qui se dédouble, en quelque sorte pour former des *vuides contigues*

 les

les unes aux autres." This is admirably describing them. Those in the water-lily are round as in fig. 2. in the Journal just mentioned, and the square sort are found in the vail of the same plant. The water-lily is in the present print. (See fig. 5, GG.)

To Messrs. Nicholson and Tilloch.

XX. *An Attempt to determine the definite and simple Proportions, in which the constituent Parts of unorganic Substances are united with each other.* *By* JACOB BERZELIUS, *Professor of Medicine and Pharmacy, and M.R.A. Stockholm.*

[Continued from p. 54.]

2. *Alumina.*

IN order to be able to determine the quantity of oxygen contained in alumina, I dissolved some of this earth, which had been precipitated from alum by caustic ammonia, in sulphuric acid; and when the acid would take up no more of it, I filtered the solution, concentrated it by evaporation, and precipitated the neutral salt from it by the addition of alcohol. The precipitate was well washed with alcohol, in order to separate any uncombined sulphuric acid which might accidentally be present. The salt thus prepared had completely the taste of alum, but the taste was much stronger. In order to drive off the water, I heated it in a platina crucible over a spirit lamp, and weighed the crucible from time to time; when it lost no more of its weight, I considered the salt as free from water. It melted, swelled up, and exhibited the appearance of alum; at last I was obliged to force it down into the crucible, in order that the heat might be able to penetrate it uniformly. The dry salt appeared not to be soluble in water, and in this respect resembled the dry sulphate of the protoxide of iron, the dry sulphate of magnesia, burnt alum, and other salts. But with the assistance of heat it was by degrees completely dissolved.

I ignited 10 grammes of this dry sulphate of alumina in a platina crucible, as long as they lost any weight from the extrication of sulphurous acid; they left behind 2·9934 gr. of a loose white and light alumina. This salt, the *dry sulphate of alumina*, must therefore consist of

Sulphuric acid ..	70·066	100·000
Alumina	29·934	42·722

And if 42·722 parts of alumina contain 19·96 of oxygen, 100 parts must contain 46·726.

In order to prepare *the hydrate of alumina,* I first attempted to

to separate the earth from alum by adding ammonia in excess. But in this I could not succeed; for, when I burnt the alumina thus obtained, it always afforded first water, and then sulphurous acid and oxygen gas. Consequently alumina forms with the sulphuric acid an insoluble subsalt, which is but imperfectly decomposed by ammonia.

I therefore took the alumina which had been freed from sulphuric acid by complete ignition, dissolved it by long digestion in nitric acid, and precipitated it by adding to the solution caustic ammonia in excess. The gelatinous earth was well washed on a filter, and slowly dried in the sun: when dry, it was rubbed to a fine powder, again digested with water, in order to separate the nitrate of ammonia, once more dried, and ignited in a small glass retort. At first pure water was evolved, but it was followed by a quantity of nitrous acid vapours, which could only be completely expelled by a white heat. Consequently the nitric acid possesses the same property as the sulphuric, of affording with alumina a subsalt, which is not completely decomposed by ammonia. The *subnitrate of alumina* has a considerable resemblance to the gelatinous silica, or to a stiff decoction of starch, and is easily obtained by rubbing unburnt alumina with a little nitric acid: the mixture exhibits after a few moments an inflated starch-like mass.

I now dissolved alumina in *muriatic acid*, precipitated it with ammonia in great excess, and digested the precipitate for six hours with the solution, which remained strongly alkaline. The earth, when taken out of the filter, and well washed, was dried in the sun, finely powdered, and again exposed to the sun for a day. When ignited in a small retort, it afforded nothing but water, although a small portion of the earth itself was carried up with the water, and was collected like a fine dust in the receiver. The loss was somewhat increased by this circumstance.

This compound of *water and alumina* left 64·932 per cent. of earth, which, being dissolved in nitric acid, showed no signs of sulphuric acid when examined by the test of a salt of baryta. Consequently 100 parts of dry alumina had been united with 54 of water: and this water contains 47·65 parts of oxygen: the alumina, on the other hand, as we have seen, contains only 46·726. I cannot insist on the perfect accuracy of either of these determinations; but both of them are sufficiently near to the truth, to show that alumina, like the bases of salts already mentioned, is capable of combining with a quantity of water of which the oxygen is *equal* to that of the earth.

Alumina, after ignition, attracts moisture very rapidly from the air, but retains it with a slight force; and the quantity depends on the hygrometrical state of the air. The warmth of the

the sun is sufficient to expel the greatest part of this water; and if we heat the alumina on a sand-bath, the whole of it is driven off; while the water in the true hydrate requires a red heat in order to be expelled. After some days, 100 parts of burnt alumina had absorbed 34·5 of water in an atmosphere saturated with moisture, hygr. 100°. Of this water it lost in a few days 18·5 parts, when the hygrometer stood at 7°, and the thermometer from 22° to 25° [72° to 77°]; and its weight then remained stationary. It may be questioned in what state of combination this water was retained: certainly not in the same as in the hydrate.

3. *Silica.*

The silica, which is separated by acids from the *liquor silicum*, contains, after being dried, as is well known, a considerable quantity of water. I found that it made no difference whether I employed silica that was precipitated at once, or that was first separated in a gelatinous form.

Three portions of silica, which I had obtained by different analytical operations, were dried in the same saucer on a sand-bath. When I ignited them after some hours' exposure to this heat, they all lost a quantity of water, which varied from 11·2 to 11·3 per cent.

The experiments upon the ignited alumina, and others upon the oxide of tin, which I shall hereafter relate, determined me to repeat these experiments at a subsequent time. I found that the oxide of tin, when dried in different temperatures, retained different proportions of water. I therefore weighed a portion of silica dried in the open air, and dried it again in a sand heat: the loss amounted to 26·8 per cent. Being left in the scale, its weight increased by degrees. I then dried it again very thoroughly, and ignited it; and I found the loss 14·2 per cent. It formed little transparent grains, which lost nothing of their transparency nor of their form by ignition. It seems, therefore, as if the water retained by the silica were precisely in the same state as that which ignited alumina absorbs from the air.

I had entertained hopes of being able to calculate the quantity of oxygen in silica, from that of the water retained in combination with it in a dry form; and the agreement of the three first experiments with each other made me suppose, that silica probably contains four times as much oxygen as the water combined with it; and in this case the quantity would be 45 per cent. But I no longer place any dependence on this mode of determination. It may perhaps be possible, at a future time, to ascertain the composition of silica from its combinations with the fluoric acid, or with the alkalis and the earths.

4. *Oxide*

4. *Oxide of Iron.*

The experiments of Mr. Liedbeck have for some time made it known in Sweden, that the yellow or brownish ore of iron called Meadow ore, "Rasenerz," contains the oxide in the form of a hydrate. My friend Mr. Hausmann of Cassel [now Professor of Technology at Gottingen, G.] wrote to me some months since that he had made the same remark, and had found from 19 to 21 per cent. of water in this hydrate, but considered the former number as the more correct. In this case the water would contain a quantity of oxygen amounting to two-thirds of that of the oxide of iron, or the water would contain as much oxygen as had been required for converting the given quantity of iron into a protoxide. Although this does not agree with the laws which have been laid down, it is evident that this latter view of the subject may lead to very instructive consequences; and I was induced by it to add to these investigations the examination of the hydrate of the oxide of iron.

Mr. Liedbeck had found, in the ores which he examined, 20·8, 21·1, or 25 per cent. of volatile substances, of which water constituted about 20. Together with the oxide of iron, he found mechanical mixtures of sand, clay, silica, and so forth, which being deducted from the oxide, left from 60 to 62 per cent. only: and this portion of pure oxide contains as much oxygen as the water combined with it.

I now examined some foreign specimens of this ore, and found in it 14·4, 13·1, 11·6, . . . per cent. of water, accordingly as the ore was dried in the sun, or in a sand heat. The ore was not magnetic before ignition, but became more or less so after ignition. Hence it must have contained a little combustible matter, which must have increased the loss by ignition. When the oxide was dissolved, after ignition, in muriatic acid, it left behind a little silica, swollen up into a semi-gelatinous form, which consequently appeared to have been chemically united with the oxide. But the admixture of foreign substances was such as to render it impossible to determine the composition of this triple combination of water, silica, and oxide of iron, with so much precision, that the result might be of the least utility as a basis for calculation.

I now examined the yellow mass which is formed on weathering pyrites, and which I had often found to be free from sulphuric acid. The specimen taken from one piece lost during ignition 17·5 per cent.; from another 12 only. Both contained silica, and the first I afterwards found to exhibit traces of copper.

I then prepared some hydrates of iron, by decomposing the sulphate, the nitrate, and the muriate of the oxide, with caustic ammonia.

ammonia. All these solutions afforded however a mixture of hydrate and subsalt, from which heat expelled first some water, and then some of the acid. The ignited hydrate lost in these experiments from 27 to 18·5 per cent.

I next digested the precipitate obtained from the nitrate with a great excess of caustic ammonia. The hydrate, when washed, and dried in the sunshine, now lost 22·15 per cent.: the fluid which escaped, was not pure water, but a strongly ammoniacal fluid. I had not therefore yet obtained a pure hydrate.

Two years and a half before, when I was examining the iron which contained silicium, I had left about 20 grammes of this iron, moistened with water, to oxidate spontaneously. But the mass assumed by degrees a solid consistency, and even after the expiration of this time, was not completely oxidated throughout its substance. Not being able therefore to employ it for the analysis of silica, as I originally intended, I collected and dried a quantity of the yellow ochre that was formed on it: but accidentally this substance was placed, with others that were to be dried, in a sand heat, and here in all probability lost a part of the water that was chemically combined with it; for the hydrates of iron support but little heat, without assuming a darker colour, and losing part of their water. By ignition, its weight was further diminished 10 per cent. The red oxide that was left contained 8·2 per cent. of silica. I was therefore working again upon a triple combination of silica, oxide of iron, and water. The quantity, which I had to employ for this experiment, was too small; and was already consumed, so that I could not examine this preparation of iron with greater accuracy: but it was evidently an artificial combination resembling the foreign ore which I have already mentioned. This ore will undoubtedly become the subject of further investigation, and the comparative analysis of the artificial triple combination, obtained by the oxidation of iron containing silicium, will perhaps be indispensable; for the ore is mixed in various and inconstant proportions with dust, and with different earthy substances, so that it can afford no certain results.

It is also obvious from what has been related, that it it difficult to obtain a perfectly pure hydrate of iron, since the oxide combines, at the moment of precipitation, as well with acids as with ammonia, according to the degree of excess in which this alkali is present. I therefore allowed some iron filings to be oxidated in pure water, which I changed daily, and collected the hydrate. After some weeks I had obtained enough for an experiment on a small scale. I dried it in the sunshine for several days, and then ignited it in a platina crucible. It left 85·2 per cent. of red oxide, which was partially magnetic; a circumstance

which

which I attribute to some carbon contained in the iron, which was mixed with the hydrate. This quantity of oxide contains 26·12 parts of oxygen, and the water combined with it 13 parts. Consequently in this experiment the oxide contained *twice* as much oxygen as the water.

But in order that this proportion might have more than a single experiment for its support, I took some iron which contained less carbon, choosing for this purpose some harpsichord wire, n. 10; I suffered it to be oxidated in pure water, which was daily changed, in contact with a plate of platina, which accelerated the oxidation. After some weeks, I had again collected enough for examination. This hydrate, dried for several days in the sunshine, had a very light orange yellow colour, and left after ignition a fine red oxide, not at all magnetic, which amounted to 85·5 per cent. Consequently in this experiment the hydrate had contained 14·5 per cent. of water, which confirms the preceding experiment; and we see that the hydrate of iron, which forms on iron in water, contains a quantity of water of which the oxygen is equal to *half* that of the oxide. It is however probable, as well from Mr. Liedbeck's experiments, as from others which will hereafter be mentioned, that the oxide of iron, when *in combination with other substances*, is capable of taking up a quantity of water, of which the oxygen is *equal* to that of the oxide.

5. *Oxide of Tin.*

Ten grammes of pure tinfoil were oxidated in a glass flask by means of pure nitric acid; the fluid was evaporated, and the oxide ignited in the flask. It was of a light straw colour, and weighed 12·72 gr. In another experiment I obtained only 12·71 gr. According to the former experiment, the *oxide of tin* consists of

Tin	[78·62]	100·0
Oxygen	[21·38]	27·2

It is a known fact that oxide of tin, prepared with nitric acid, reddens litmus paper, but not after ignition. I thought at first that this might depend on some nitric acid adhering to it; but when the water, with which it was washed, no longer reddened the paper, the oxide still possessed the property. It was only deprived of it by pouring on it a little ammonia; but the oxide now afforded, when ignited, water strongly impregnated with ammonia. The oxide of tin therefore has as much right to the denomination of an *acid* as the tungstic acid, and the columbic acid, or rather the "tantalic" oxide.

I dried some oxide of tin, which had been well washed, but not treated with ammonia, in a sand heat. It lost by ignition 6·6 per cent. After some hours, when the sand-bath was considerably

siderably colder, I repeated the experiment with a part of the same oxide, which had remained on the sand. It now lost 9·66 per cent.: another portion, left for a longer time, lost 10·8 per cent. by ignition, and the next morning, when every thing had been completely cold for some hours, the loss amounted to 12·5 per cent.; the weather being in the mean time very damp and rainy. It is impossible to determine which of these experiments ought to be preferred: and although this oxide, which approaches so nearly to the acids in its nature, when it retains the greatest portion of water, contains itself nearly twice as much oxygen as this water, still we can form no very decided conclusion from this circumstance.

It may be hoped that similar experiments on the water contained in the oxides of titanium and tantalium, on the tungstic acid, and other similar substances, will lead us to a more general and decided knowledge of the relation of water to these bodies, which occupy a middle rank between acids and bases.

But what are we to think of the attraction of a body for water, which retains this fluid so slightly as to be overpowered by its expansive force at a moderate temperature? It must manifestly be the same power that causes filtering paper to attract moisture from the atmosphere, and sometimes to become heavier even during the operation of weighing it, and that exhibits the well known effects of hygroscopical substances. Whether this force differs only in degree from that which is characterized by regularly defined relations between the bodies that are united, or whether it merely consists in a modification of superficial attraction, I do not venture to decide. With respect to the modifications of the force of elective attractions, there still remains much for us to investigate. for example, in what the force which causes a salt to be dissolved in water, in quantities dependent on the temperatures, differs from the force by which the same salt is capable of rendering solid a greater or less quantity of water of crystallization. Between these quantities there is no determinate and unalterable relation; for salts, which contain no water of crystallization at all, may be very soluble in water, for example, nitre, and muriate of potass; while others, which contain it very abundantly, are completely insoluble, as, for example, subsalts of iron and copper, carbonate of magnesia, and some others.

C. *Combinations of Water with Salts.*

Water of Crystallization.

Salts of potass. In no salt of potass that I have examined, the sulphate, nitrate, muriate, nor tartrate, have I found a trace of water of crystallization. If these salts are finely powdered, and dried in the sunshine, or in any other moderate warmth, they

they lose no more water during ignition. The nitrate of potass gives off in the operation oxygen gas, nitrous acid, and lastly nitrous gas, but does not afford a drop of liquid acid. The tartrate of potass, precipitated with nitrate of the protoxide of lead, afforded, for 100 parts of the well dried salt, 155·7 of tartrate of the protoxide of lead. Consequently, according to the analysis of this salt lately related, the *tartrate of potass* consists of

Tartaric acid . .	58·69	100·0
Potass	41·31	70·4

Now 70·4 parts of potass contain 11·93 of oxygen, that is, with a very slight difference, the same quantity as the protoxide of lead that saturates 100 parts of the acid. The slight difference may depend on the unavoidable loss of a small quantity of tartrate of lead in the experiment. Hence it is evident, that the tartrate of potass can contain no water of crystallization.

The supertartrate of potass, on the contrary, or the crystals of tartar, contains water of crystallization, which cannot be expelled by heat. The salt employed for the preceding experiment had been made of pure tartaric acid with pure carbonate of potass, and was therefore perfectly free from lime. I precipitated the remaining portion of it with tartaric acid, I dried very carefully the powdered precipitate, and burnt 10 grammes of it in a platina crucible. The coaly alkaline mass was carefully washed with muriatic acid, the muriatic solution dried, and the salt ignited. I obtained in one experiment 3·91, and in another 3·915 gr. of muriate of potass. I found by another experiment, as Wollaston had already observed with respect to the oxalic acid, that the residuum of 10 gr. of supertartrate of potass, after ignition, was sufficient to saturate 10 gr. of the supersalt: so that the potass must be combined with twice as much acid in the supersalt as in the neutral salt. But the muriate, which was obtained, answers precisely to 24·8 per cent. of pure potass, which must consequently form the superacetate with 70·45 of tartaric acid, and the remaining 4·75 parts must be water. Hence the superacetate of potass consists of

Tartaric acid . .	70·45
Potass	24·80
Water	4·75

This quantity of potass contains 4·206, and the water 4·192 parts of oxygen; consequently the water of crystallization in this salt contains exactly *as much* oxygen as the base. But since this water can only be expelled by the addition of a second base, and is exactly the same quantity as would have been combined with the excess of acid in a separate form of crystallization, this salt may be considered as a double salt, of which water is the second base.

Salts

Salts of soda. While, according to the experiments which are here related, the salts of potass seem in general to be without water of crystallization, those of soda on the other hand contain it in abundance, but retain it with a very slight attraction, so that the greater number of them fall to powder in a dry atmosphere. Hence it is very difficult to obtain accurate results with respect to them, since it may easily happen, that such a salt may begin to crumble away at the surface, before it is dry in the middle.

Sulphate of soda, in pure crystals, was washed with water, pulverised, and in great measure separated again from the water on blotting paper, then dried in a press for twenty-four hours, between several folds of fresh blotting paper, and lastly dried again by slow degrees, and then ignited, in a platina crucible. In this process 30 grammes lost 16·8 in weight; consequently the salt contains 56 per cent. of water of crystallization, and must consist of

Sulphuric acid . . .	24·76
Soda	19·24
Water	56·00

Now 19·24 parts of soda contain 4·953 of oxygen, and 56 of water 49·42; so that in this salt the water of crystallization contains exactly ten times as much oxygen as the base.

Acetate of soda. Ten grammes of acetate of soda, powdered, and dried in the open air, when exposed to the heat of a sand-bath, fell into dust, and lost 4·011 gr. of their weight. Five grammes of the acetate which had crumbled to dust, when muriatic acid was poured on it, and the salt was dried and ignited, afforded 3·584 of muriate of soda. Consequently the acetate of soda must consist of

Acetic acid	61·689	100·0	36·95
Soda	38·311	62·1	22·94
			40·11 Water,
	when dry.		when containing water.

Now 22·94 parts of soda contain 5·897, and 40·11 of water 35·397 parts of oxygen; and $5 \cdot 897 \times 6 = 35 \cdot 382$. Consequently in this salt the water of crystallization contains six times as much oxygen as the base.

Acetate of lime. In order in some measure to check this result, I converted 10 gr. of acetate of lime, which had been dried to a powder in a strong heat, into muriate of lime, and obtained from it 7·005 gr. which contained 3·5782 of pure lime. Consequently the acetate of lime consists of

Acetic acid . .	64·218	100·00
Lime	35·782	55·74

The quantity of soda, which decomposes 100 parts of acetate of

of lime, contains 15·89 parts of oxygen, and the quantity of the lime contained in 100 parts of this salt contains 15·71 of oxygen. Consequently this analysis appears to confirm the former, although they do not agree in the last figures; but they show at least that the quantity of oxygen required in a base, by 100 parts of acetic acid, cannot be materially different from these numbers.

Muriate of ammonia. It appears from my analysis of this salt already related, that it consists of

Muriatic acid......	50·86
Ammonia.........	31·95
Water...........	17·19

[Since lime detaches 31·95 per cent. of ammoniacal gas from it, and nitrate of silver indicates 50·86 per cent. of muriatic acid; the rest being water. G.] But in 31·95 parts of ammonia there are 14·98 of oxygen, and in 17·19 of water there are 15·17 of oxygen. Consequently the water of crystallization of this salt must contain as much oxygen as the base. If the composition of water were ascertained with so much certainty, that we could depend on the last decimals of the determination, the result would indicate a small error in the analysis of the muriate of ammonia, and it would appear that I had found $\frac{19}{10000}$ too little ammonia in it. But it is scarcely worth while to attempt to make this correction; and in other determinations, for instance, those of the muriatic acid, we cannot depend on the decimals. If however none of the analyses, which I have published in this Essay, are perfectly correct, except by accident, still they appear to me to come near enough to the truth to allow us to employ them with safety in the investigation of the laws of the proportions of mixtures. It will only be possible to undertake the difficult task of discovering perfectly correct numerical expressions for these proportions, with better prospects of success, when these laws have been previously so well established, as to allow us to employ them with confidence in our analyses. [In this pursuit we may derive sufficient encouragement from the example of astronomers, who would never have been led by observation alone to the wonderful precision which the theory of the science has attained; and Mr. Berzelius, if I am not mistaken, will enjoy the lasting honour of having been the first [?] that has observed the possibility of following a similar path in chemical science, and not only of having pointed out this path, but of having already conducted us a considerable way on in it, by the masterly analyses which he has performed in this investigation. G.]

Of the *nitrate of ammonia* I have spoken in the Second Continuation of this Essay, which related to the capacity of saturation and the true composition of the nitric acid; and I have there shown that, like the muriate, it contains a quantity of water

of crystallization of which the oxygen is *equal* to that of the base.

Sulphate of ammonia. I had mixed 10 grammes of dry sulphate of ammonia with 30 gr. of caustic lime in a small glass retort, and then completely filled up the bulb and the neck of the retort with lime. I fitted to the mouth of the retort a small glass tube filled with caustic potass, and then heated the bulb slowly till it was completely ignited. When no more ammoniacal gas escaped through the opening of the tube, I withdrew the heat: the apparatus, when cold, had lost 2·26 gr. of its weight. In another experiment it lost only 2·25 gr. Now since the ammonia must saturate a quantity of sulphuric acid which contains three times as much oxygen as itself, this salt must consist of

Sulphuric acid	53·1
Ammonia	22·6
Water	24·3

This quantity of water contains 21·444 of oxygen, and the ammonia, according to my determination, 10·6; and $10{\cdot}6 \times 2 = 21{\cdot}2$.

In these experiments it is very difficult to drive out the ammonia completely, for a small portion of it almost always remains in the water which is condensed in the lime and the potass. But we see from the experiment, that in this salt the water of crystallization contains *twice* as much oxygen as the base.

The *oxalate of ammonia* exhibits the singular spectacle of an ammoniacal salt falling to powder. According to some former experiments, the loss of weight during this efflorescence should amount to 16 per cent. But in several experiments I never obtained a greater diminution than 13·75 per cent. If now 100 parts of oxalic acid saturate a base which contains 21·2 parts of oxygen, the salt must be thus constituted:

Oxalic acid	59·37
Ammonia	26·88
Water	13·75

This quantity of ammonia contains 12·586 of oxygen, and the water 12·134. We may therefore assume it as demonstrated, that the base and the water of crystallization in this salt contain *equal* quantities of oxygen. The variation certainly depends only on some slight errors in the experiments.

Muriate of baryta. Seventeen grammes of finely powdered muriate of baryta, dried in the shade, lost 2·505 gr. of their weight, when ignited in a platina crucible. Consequently the crystallized salt consists of

Muriatic acid	23·349
Baryta	61·852
Water	14·799

This

This quantity of baryta contains 6·495, and the water 13·05 parts of oxygen. But 6·495 × 2 = 12·99. The water of crystallization in this salt contains therefore *twice* as much oxygen as the base.

Sulphate of lime. From the analysis of crystallized gypsum, described by Bucholz, we know that it is composed of the following parts:

Sulphuric acid	46
Lime	33
Water	21

This quantity of lime contains 9·29 of oxygen, and the water 18·53. Now 9·29 × 2 = 18·58. Consequently, the water in this salt contains *twice* as much oxygen as the base.

Muriate of lime. I had collected and put by some years ago a quantity of fine crystals of muriate of lime. I pounded them quickly, and dried them between many folds of blotting paper, several times changed, in a press, until the salt communicated no more moisture to the paper. I weighed 10 grammes of the salt thus dried, in a well-stopped glass flask, then heated them in the same flask open, and lastly ignited them. The salt had lost 49·603 per cent. of its weight. The crystallized muriate of lime is therefore thus constituted:

Muriatic acid	24·686
Lime	25·711
Water	49·603

Now 49·603 parts of water contain 43·774 of oxygen, and 25·711 of lime, 7·24; and 7·24 × 6 = 43·44. It is easy to see that a small excess of water could not here be avoided. Consequently the water of crystallization contains *six* times as much oxygen as the lime.

Sulphate of the protoxide of iron. Ten grammes of the sulphate of the protoxide of iron were very strongly heated in a glass retort, but not raised to a red heat: they lost 45·4 gr. of water. This salt consists therefore of

Sulphuric acid	28·9
Protoxide of iron	25·7
Water	45·4

The oxygen of the water amounts to 40·16, and that of the protoxide to 5·8; and 5·8 × 7 = 40·6. Hence we see that the water of crystallization of this salt contains seven times as much oxygen as the base.

Sulphate of the protoxide of zinc. Ten grammes of this salt, being heated over a spirit lamp in a platina crucible, lost 36·45 per cent. But since the dry salt consists of 49·52 parts

of

of sulphuric acid and 50·48 of the base, it contains, when crystallized,

Sulphuric acid	30·965
Protoxide of zinc ..	32·585
Water	36·450

The base contains 6·39, and the water 32·15 parts of oxygen; but $6{\cdot}39 \times 5 = 31{\cdot}95$: consequently the water contains *five* times as much oxygen as the base.

Sulphate of the oxide of copper. Ten grammes of this salt in crystals lost, when dried to a powder over a spirit lamp, 36·3 per cent. in weight. The crystallized sulphate of the oxide of copper consists therefore of

Sulphuric acid	31·57
Oxide of copper ...	32·13
Water	36·30

since, according to the corrected analysis of the sulphate of baryta, 5 grammes of the sulphate of copper consist of 2·477 gr. or 49·55 per cent. of sulphuric acid, and 50·45 per cent. of oxide of copper, or 101·82 parts of oxide of copper for 100 of sulphuric acid. The oxygen of the oxide of copper, being computed at 24·5 per cent. amounts to 6·32, and that of the water to 32: but $6{\cdot}32 \times 5 = 31{\cdot}6$; so that the water of crystallization contains *five* times as much oxygen as the base.

Nitrate of the protoxide of bismuth. Ten grammes of this salt, crystallized, and dried in the air after being powdered, were heated in a small retort to complete ignition. They afforded in one experiment 5·13, and in another 5·12 grammes of liquid nitric acid, not smoking. Consequently this salt must contain more water than is sufficient to exhibit the acid in its highest state of concentration. According to the very accurate experiments of Mr. Lagerhjelm, the protoxide of bismuth contains 10·13 per cent. of oxygen; 48·8 parts must therefore contain 4·9434, and must consequently saturate 33·7 parts of nitric acid, [since 100 parts of nitric acid suppose $14\frac{2}{3}$ of oxygen in the base which saturates them. G.] The remaining 17·5 parts, which make up the 100, must have been water. But these contain 15·4 of oxygen; and $4{\cdot}9434 \times 3 = 14{\cdot}83$. We see therefore that, according to this experiment, which cannot be very accurate, from the readiness with which the atmospherical moisture decomposes the salt, the water of crystallization must contain *three* times as much oxygen as the base.

Conclusions.

I consider these examples, for which salts of so different natures have been selected, as sufficient to demonstrate the law, that "the oxygen of the water of crystallization is always an integral

tegral multiple, or, as in the cases of the citric acid, and the subcarbonate of the oxide of copper, an integral submultiple of the oxygen of the base."

If we compare the oxygen of the water of crystallization with that of the acid, we do not always find the same law observed: thus, in the sulphate of soda, for example, the oxygen of the water of crystallization is to that of the acid as $3\frac{1}{3}$ to 1, and in the sulphate of ammonia it is $\frac{2}{3}$ of that of the acid. Hence I have been led to the following rule: "In combinations of several oxygenized bodies, the oxygen of the component part, which contains the least of it, is an aliquot part of that of each of the other component parts."

But may not we discover, in combinations of this kind, a *prevalent* component part, of the oxygen of which, although its quantity may not be the smallest, that of the other parts must be multiples or submultiples? If, for example, in a subsalt, the acid contained one-third, and the water half as much oxygen as the base, the oxygen of the two former would stand in a regular relation to that of the latter, which might be regarded as the prevalent component part; but the oxygen of the water would not be a multiple of that of the acid, which in this case would be the least of the three. Among the few substances which I have had occasion to examine, I have met with no instance of this kind; but it does not follow that the latter view may not be a correct one. Since however every combination depends on the whole sum of the attractions of each of its component parts, the former mode of representation appears to me to be the more probable.

I confess that we have at present too little experience to be able to set up such inferences as demonstrated laws. But we are obliged to adopt them at least as temporary suggestions, in order to our advancement towards a more certain theory: and I am convinced that all the opinions which I have here expressed will in general be confirmed as truths; although they may require numerous corrections, in proportion as more and more of the infinity of matter, which has hitherto eluded our researches, shall be made known by future investigations.

[To be continued.]

XXI. *New Outlines of Chemical Philosophy.* By Ez. Walker, *Esq. of Lynn, Norfolk.*

[Continued from p. 25.]

Combustion.

There are very few bodies in nature, that do not contain either thermogen or photogen. All those bodies which are called com-

 bustibles

bustibles contain the element of light, which I have been induced to call photogen to avoid the use of ambiguous terms; and all those bodies, which are called supporters of combustion, contain the generator of heat, called thermogen for the same reason*.

These elements may be made to unite and produce combustion, or increase the temperature of bodies, by various means, as: 1st, by friction; 2dly, by percussion; 3dly, by pressure; 4thly, by mixture; 5thly, by increase of temperature; and 6thly, by the functions of living animals and vegetables.

Thermogen and photogen in a condensed state, as in a charged Leyden jar, have so strong an attraction for each other, that they will pass through any of those bodies called conductors, to a very great distance, to be united. But, when they are united to ponderable matter, they will remain mixed together for ages without producing any effect; but as soon as they are brought within a certain distance of each other, which may be called their striking distance or their sphere of action, either combustion takes place, or the temperature of the body, in which they meet, is increased.

The following explanations of some well known experiments will tend to show the importance of a theory founded on *facts*.

Experiment 1. In a Memoir read to the National Institute of France, M. Biot announces the important fact, that a mixture of hydrogen and oxygen gases may be made to explode by mechanical compression only, independently of the electric spark.

Explanation. Oxygen and hydrogen gases would remain mixed together, for any length of time, without combining, the thermogen and photogen which they contain being kept at too great a distance by their bases; but by mechanical compression only, they are forced within their striking distance, and light and heat are produced on the same principle as combustion is produced by discharging a Leyden jar.

Experiment 2. "Take six grains of oxygenized muriate of potass, and three grains of flour of sulphur, rub them together in a mortar, and a smart detonating noise will be produced. Continue to rub the mixture hard, and the report will be frequently repeated, accompanied with vivid flashes of light. If the same mixture be wrapped in paper, laid on an anvil, and smartly struck with a hammer, the report will be as loud as what is usually produced by a pistol."

Explanation. These experiments may be explained on the same principle as the last. The two ingredients, though mixed together, produced no effect until the thermogen in one of them and the photogen in the other were forced into union, by friction or percussion. And on the same principle the effects of many other fulminating substances may be explained.

* See page 23.

Experiment

Experiment 3. "If an ounce of strong nitrous acid be mixed with about half its weight of sulphuric acid, and poured into a little oil of turpentine, the whole will immediately burst into flame."

Explanation. The thermogen and photogen contained in these fluids coming immediately into contact, flame is produced instantaneously, without any mechanical force or increase of temperature.

Now, from this experiment it appears impossible that water should be a compound of oxygen and hydrogen; for, if it were, these two principles of combustion would burst into flame like the acid and the oil. But no pressure, no increase of temperature, nor any union with combustible matter, can produce a single spark of fire from water. In short, nothing combustible is found in its composition.

Experiment 4. Combustion may also be generated by friction. Two pieces of hard wood, being smartly rubbed together with great pressure, will soon burst into flame.

Explanation. The first increment of caloric, by friction, is excited by a portion of the thermogen of the air being forced into union with the photogen in the wood; and by continuing the operation, the temperature of the inflammable principle of the wood is increased till flame is produced.

Experiment 5. When a rod of iron is laid upon an anvil and hammered with a quick succession of heavy strokes, it soon becomes red hot; but it cannot be strongly heated a second time by the same means, unless it has been previously introduced into a fire.

Explanation. The first stroke of the hammer forces a small portion of the thermogen of the air into contact with the photogen of the metal, which generates the first increment of caloric; and by repeated strokes of the hammer the caloric is increased, till the rod becomes red hot. But the photogen of the metal becomes exhausted by this means, and cannot, therefore, produce caloric a second time, till it has recovered its combustible property in the fire.

The burning of iron wire in oxygen gas shows, in a very brilliant manner, how iron loses its photogen and becomes oxidized.

Experiment 6. "Mr. Thomas Wedgwood has shown that it has never yet been explained how friction produces caloric.

"He took a piece of common window glass, and held the edge of it against the edge of a revolving grit-stone, and that part in contact with the stone became red hot, and threw off hot particles which fired gun-powder. The stone and the glass being both incombustible substances, it remains to be explained how caloric was produced."—*Philosophical Transactions for* 1792.

Explanation. It has been shown in a former paper, that the electric machine creates nothing, but by friction only it collects the two elements which produce caloric from the earth and the atmosphere. Although the stone and the glass, in Mr. Wedgwood's experiment, are both incombustible; yet it is almost a self-evident conclusion, that they collected the two elements of caloric from the air and the earth by friction in the same manner as the electric machine.

If this explanation be not sufficiently conclusive, let two small pieces of plate-glass be rubbed together for a short time, and they will show electrical phænomena. The two pieces of glass being incombustible, the elements which produce those phænomena must come from the earth and the atmosphere. This experiment is sufficient to show us how combustion was produced by the pressure of the glass upon the grit-stone when it was revolving with great velocity.

Experiment 7. "It is well known, that when flint and steel are smartly struck against each other, a spark always makes its appearance, which is capable of setting fire to tinder or to gunpowder. The spark in this case, as was long ago ascertained by Dr. Hooke, is a small particle of the iron, which is driven off, and catches fire during its passage through the air. This, therefore, and all similar cases, belongs to the class of combustion. But light often makes its appearance when two bodies are struck against each other, when we are *certain* that no such thing as combustion can happen, because both the bodies are incombustible. Thus, for instance, sparks are emitted when two quartz stones are struck smartly against each other, and light is emitted when they are rubbed against each other. Many other hard stones also emit sparks in the same circumstances.

If they be often made to emit sparks above a sheet of white paper, there are found upon it a number of small black bodies, not very unlike the eggs of flies. These bodies are hard but friable, and when rubbed on the paper leave a black stain. When viewed with a microscope, they seem to have been melted. Muriatic acid changes their colour to a green, as it does that of lavas. These substances evidently produced the sparks by being heated red hot*.

Explanation. It may be necessary to observe, in the first place, that if the flint be moved over the steel very slowly, no spark will be produced, nor will any spark appear if the flint barely touches the steel, although the velocity of the flint be the greatest possible: consequently caloric cannot be produced by collision or friction, but by velocity and pressure conjointly.

* Thomson's Chemistry, vol. i. p. 421.

Secondly,

Secondly, by the collision of the flint and steel, the thermogen of the atmosphere is forced into union with the photogen of the metal. The combustion thus produced, melts the minute particles of steel struck off by the flint into small globules. That this metal contains the generator of light is well known, by the burning of iron or steel wire in oxygen gas.

Sparks are emitted when two quartz stones are struck smartly against each other under water; and Mr. Kirwan affirms, that sparks are produced by the collision of flint and steel under common spring water.

That sparks are produced by the collision of flint and steel under water, I have no doubt, since water is *a better* conductor of thermogen and photogen than *atmospheric air*. And the sparks so produced, being intensely hot, cannot be extinguished by the water* as soon as they are generated.

Experiment 8. From Count Rumford's experiments it appears, that by a moderate degree of friction the same piece of metal afforded so much caloric under water as to keep it boiling.—*Phil. Trans. for* 1798.

This experiment may be explained on the same principle as the last. Water, being a *better conductor* of thermogen than *atmospheric air*, cannot prevent the production of caloric by the collision or friction of hard bodies.

Lynn, Feb. 4, 1814. EZ. WALKER.

XXII. *On Sir* H. C. ENGLEFIELD's *new Transit Instrument. By the Rev.* JAMES GROOBY.

Cirencester, Feb. 4, 1814.

SIRS,—ON receiving your last Magazine, I was not a little surprised to see in the new Transit Instrument there described, the exact representation of one I had thought of more than three years ago; a drawing and description of which I then sent to Mr. Banks, astronomical instrument-maker, &c. Strand, requesting him to make me a transit upon that plan, unless he saw very good reasons to suppose it would not answer. He wrote to me in return, that he had well considered my plan, had shown it to several scientific gentlemen (among whom he particularly mentioned Mr. Pond, the present astronomer royal), that they had all pronounced such a construction of the instrument useless, and he therefore by all means recommended one on the old plan, and not to put myself to a needless expense about an instrument

* When iron or steel wire is burnt in oxygen gas, the particles of metal which drop from the end of the wire will fuse the glazing of an earthen plate, after having passed through water an inch deep.

that

that never could answer the purposes for which it was intended. Not content with this rejection *in toto*, without any particular fault being pointed out, and seeing Mr. B. so prejudiced against it, I determined to attempt the constructing of it myself; and, rude as my instrument was, its performance convinced me that it had not merited the unqualified censure that had been passed upon it. Since that time, I have been procuring glasses, &c. for a more perfect essay on a larger scale; and it is something extraordinary that Mr. T. Jones himself, about a month ago, furnished me with the apparatus for adjusting the wires, but without my mentioning to him the instrument it was intended for. That Sir H. C. Englefield was one among the scientific gentlemen to whom my plan was shown by Mr. Banks, or that he got the hint from it in any other way, I cannot for a moment suppose, since he has mentioned the source from whence he derived his idea. It must however be considered as a very extraordinary instance of the same thought occurring to two individuals at nearly the same time; and certainly Sir Henry, in meeting with a person so competent to realise his idea, has been more fortunate than I. From the little experience I have had with the home-made instrument above mentioned, I am convinced that, when properly constructed, it is capable of the greatest possible accuracy. By having two marks, one to the north or south, and the other to the east or west, the position of the instrument may at all times be ascertained, as nearly as it is possible to place those marks at right angles to each other; and if the northern or southern mark be bisected by the central wire at the instant of observation (as it always ought to be), such observation must be as perfect as the power of vision in the instrument to distinguish when the object and wires are in contact. Nor will it be found impossible, when the object-glass is pretty large, to observe stars in the day-time. I had at first intended to subjoin a few observations, to show how far it was capable of being used by day; but as I have sent the glasses, &c. to Mr. Jones to be fitted up by him in a rather more accurate manner than I had been capable of doing, and as I expect to find it much improved thereby, I shall defer it to a future opportunity.

I am, gentlemen,

Your obedient servant,

James Grooby.

Messrs. Nicholson and Tilloch.

XXIII. *Solutions*

XXIII. *Solutions of Sir* H. C. Englefield's *Mathematical Question given in the last Number of this Journal.*

Liverpool, Feb. 4, 1814.

Sirs,—I beg leave to present you with the following solution to the question proposed by Sir H. C. Englefield, Bart. F.R.S. F.A.S. &c. in the last number of your most excellent Magazine; and at the same time take the liberty to remark, that in the stating of the question I think a small typographical error has crept; for the two *last*, read the two *first* (11th line, page 64).

All numbers under a million must consist of six figures or less, and consequently the cube roots of all such numbers must contain two figures or less. In numbers whose roots have only one figure, no difficulty can be found, as their roots can be obtained from simple inspection: the remaining case is then that of such cubes that their roots may contain two digits. In order to obtain the first digit, from the given number of figures subtract three, and of the remaining figures we by the supposition know the two first, and thus we obtain a number consisting of either one, two, or three digits: the nearest root of this number may be immediately found by simple inspection; and that root will be the first digit of the number required.

The second digit of the root may be obtained by considering that all numbers must end either in 0, or in some of the nine digits: now the cubes of these digits are respectively 0, 1, 8, 27, 64, 125, 216, 343, 512 and 729; therefore the cubes of all numbers ending in 0, 1, 2, 3, 4, 5, 6, 7, 8, or 9, must terminate in 0, 1, 8, 7, 4, 5, 6, 3, 2, or 9. From this consideration it is evident, that knowing the terminal digit of any cube, we can immediately by inspection determine that of the root, and thus the first digit being before found, we obtain the root itself.

Thus, in the example the learned Baronet has given: from the given number of figures (6) subtract three, and three will remain; we therefore, affix a cypher to the two given figures 43, and obtain 430, whose nearest root is 7. The terminal figure being 6, we find that the last figure of the root must also be 6; and therefore the root required must be 76, whose cube is 438976.

I take the liberty of proposing, for the amusement of your scientific readers, the two following questions: I think they have both been before proposed, but imagine that they admit of more interesting solutions than have hitherto appeared.

1. Required a method of determining at sight, or by simple inspection, whether any given number be a prime number or not.

2. By what methods could the Romans solve simple arithmetical questions with their mode of notation?

I remain, gentlemen,

Your obedient humble servant,

To Messrs. Nicholson and Tilloch. Egerton S. Eyres.

Chichester, Feb. 5, 1814.

SIR H. ENGLEFIELD's mathematical question published in your last number may, I believe, be solved in the following manner:

The cube root of every number under a million must of course consist of not more than two places of figures. Of these the last may be immediately determined, by observing that with regard to six of the ten integers, viz. 1, 4, 5, 6, 9, and 0, the unit's place is always the same in the cube and its root; while 2 and 8, 3 and 7, are reciprocally changed one into the other in the processes of involution or evolution.

The other figure of the root may be obtained as in the common rule for extracting the third power, viz. by deducting 3 from the given number of places, and taking the root of the cube next less than the amount indicated by the one, two, or three figures remaining on the left hand.

Thus, in the example proposed, where we have given the unit's place 6, the two first figures 43, the number of places 6;

The unit's place in the root will of course be 6, as in the cube.

Deducting 3 from the number of places, there remain 3; therefore, affixing a cypher to the two first figures, we obtain 430. The cube next less than this is 343, of which the root is 7. The root required is therefore 76.

In this way, I find I can obtain the root of any cubic number almost instantaneously; but I can hardly suppose so simple and obvious a principle to have escaped the notice of mathematicians.

B.

Paddington, Feb. 9, 1814.

SIRS,—HAVING been induced by the letter of Sir H. C. Englefield, inserted in your last, to turn my attention to the subject of his inquiry; I am enabled by the following simple process to name *immediately* the root of any cube under a million, whose two first figures, last figure, and number of figures are given.

All that appears necessary for the performance of this problem is a knowledge of the cubes of the digits. As the root consists of two figures, its tens will be indicated by, and must be sought for in, its two first figures, and its units in the last figure given.

By observing the number of figures in the cube we previously discover the *value* of the two first figures given, whether they contain the units only, or the tens and units, or the hundreds and tens of the first figure of the root when cubed: having ascertained this, we compare these figures with the cubes of the digits, and call that digit with whose cube they correspond, or to which they are next in superiority, the first figure of the root.

The last figure given, which is of course the unit of the cube, will produce the second figure, or unit of the root, by this simple rule:

rule: 1, 4, 5, 6, 9, 0, remain unaltered, while 3 and 7, 2 and 8, are respectively convertible into each other; or each when occurring in the cube may be represented in the root by what it wants of 10.

The instance adduced by Sir H. C. Englefield is a cube of 6 figures, the two first of which are 43, and the last is 6. Now as in a cube of 6 figures, the cubed tens of the root exist in the *three* first figures, these *two* first, here given, must contain the *hundreds* and *tens* of that cube, and 43 represent 430 (the amount of the unit's place being immaterial). The regular cube next below 430 is 343, whose root (7) is consequently the *first* figure required. For the *second* figure, 6 in the cube gives, as before explained, 6 in the root, and the whole root is therefore 76.

I know not if this be the method used by Sir H. C. Englefield, and shall be happy to see any others suggested by your correspondents; but must observe that this appears amply sufficient for the purpose, as by it any one, after a practice of less than half an hour, may tell the root of any number, within the bounds of the question, as soon as proposed to them.

I remain, sirs,

Your very obedient servant,

JOHN DILLON.

To Messrs. Nicholson and Tilloch.

Lansdown Crescent, Bath, Feb. 10, 1814.

SIRS,—THE solution of the following question proposed in your last Magazine is not difficult.

"Of any cube number under a million, give the figure of the unit, the two first figures, and the number of places, instantly and without any aid of writing to name its cube root."

In every cube number, two figures, the *first* and *last*, may be known by simple inspection; the first being the nearest cube root under the first or left hand period, whether consisting of one, two, or three figures, and the last being invariably indicated by the final digit. Thus, if the final digit be 2, the last figure of the root will be 8; if it be 3, the root indicated will be 7: a table of the cubes of all the single digits at once exhibits the unvarying correspondence between the final digit of the cube and its root. Now, according to the conditions of the question, the cube whose root is sought, can never exceed six places of figures: consequently the root must consist merely of a *first* and a *last figure*, which we have seen may be instantly discovered by simple inspection. It is true, the intermediate figures between the final digit and the two first figures are not given; but the number of places being given, it is (for an obvious reason) only necessary mentally

mentally to substitute noughts in the places of the unknown intermediate figures.

I shall subjoin three examples, which comprehend all the varieties of the question; and am, sirs, your obedient servant,

FRANCIS ELLIS.

Given 94...2. Here the number of places being six, the first period (according to the rule for extracting the cube root) must consist of three figures, which after the mental substitution of noughts will be 940, whose nearest less cube root is 9, and the final digit 2, indicating 8 for the remaining figure of the root: the entire root is "*instantly and without any aid of writing*" perceived to be 98.

Given 50..3. The number of places being five, the first period must consist of only two figures, which are (50), the two first given, whose nearest less cube root is 3, and the final digit 3, indicating 7 for the other figure of the root: the whole root is 37.

Given 6, 8. 9. The number of places being only four, the first period consists of the single figure 6, whose nearest less cube root is 1, and the final digit 9, indicating 9 for the other figure of the root: the whole root is at once found to be 19.

The number 438976, given by Sir H. C. Englefield, is not a cube number*.

To Messrs. Nicholson and Tilloch.

Royal Military Academy, Woolwich, Feb. 12, 1814.

SIRS,—IT gave me great pleasure to observe the sentiments with which Sir H. C. Englefield introduced his question in your last number, relative to the extraction of certain cube roots. He there justly states, that it is not common to introduce questions of this kind to the public through the medium of periodical Journals, though it was not unusual in the early part of the last century; and every one acquainted with the progress of the sciences at that period will admit that much of it was to be attributed to such communications. I made a similar remark some little time back, in proposing a question in the Philosophical Journal, under the signature of "Mathematicus;" and though I failed in my attempt to bring the subject under examination at that time, I am not without hopes that the example of a gentleman, so well known and esteemed in the scientific world as Sir H. E. is, may be followed by others, and that we may thus see, at times, a few pages of your valuable Magazine employed on subjects of this nature.

With respect to the question proposed, it is far from difficult, nothing further being necessary for its solution, than that of re-

* Probably a typographical error. Read 438996.—EDIT.

membering

membering the cubes of the nine digits. For all numbers ending in the same digit, having their cubes ending also in a constant digit, it follows that the terminating figure of the cube will immediately indicate the unit's place of the root; that is, If the terminating figure of the cube be 1, 2, 3, 4, 5, 6, 7, 8, 9,
that of the root is .. .. 1, 8, 7, 4, 5, 6, 3, 2, 9.
And with regard to the other figure of the root, or that in the tens place, it is found with equal ease from the leading figures of the cube, viz. If the three right-hand figures be supposed to be pointed off, the nearest cube root to the remaining figures will be the figure in the tens place of the root: or, which is the same, If after pointing off the three right-hand figures, those remaining exceed 1, 8, 27, 64, 125, 216, &c. the figure in the tens place will be accordingly 1, 2, 3, 4, 5, 6, &c. and which, therefore, is known as readily from the three leading figures of the cube (or from the two leading ones, if the number of them be also known) as that of the unit's place is by the termination of the cube.

Suppose, for example, the cube root of 658503 were required. The nearest integral cube root of 658 is 8, and the terminating figure being 3, that of the root is 7; therefore the root is 87. And it would have been just the same if we had only known the two first figures 65, provided we had at the same time known that there were *three* figures in the period, or, which is the same, six figures in the proposed cube; and in the same manner may the cube root of any other cube number, within the proposed limits, be immediately ascertained.

It is undoubtedly on these principles that Z. Colbourn performs his extractions of the cube root, and which he can apply with great ease to 12 figures, or to four, in the root. But in order to effect this, it is necessary for him to know the two terminating figures of the first 100 numbers, which he is perfectly master of; but whether absolutely from memory, or from a very rapid multiplication it is difficult to say. At all events, he is able to name them with great rapidity; and I have by me a table of this kind, which was set down from his dictation, and is correct in every figure.

He undoubtedly owes much to an extraordinary memory, as well as to a remarkably ready perception of the qualities and properties of numbers; of which I could mention many instances, relating to the summation of arithmetical and geometrical progressions, the naming of prime numbers, &c. In one instance, he repeated to me every prime number in its order from 1 to 600, with only one mistake, which he corrected immediately, and was very anxious to be allowed to proceed to 1000. But here it was obvious that he did not depend upon his memory only, but on a sort of computation. I know it has been stated that what he can

perform

perform in arithmetical computations is the result of tuition; and that he will not discover his methods. This however, so far as my observations have been extended, in several examinations of him both in company and in private, I can positively contradict. All his ideas were obviously intuitive, yet in most cases correct: indeed the only instance in which I found that he proceeded on false principles was in resolving numbers into their factors; and even here he would succeed in the greater number of cases.

These examinations were undertaken for the purpose of giving a sketch of his methods or rules (if they may be so called), in a work which his father intended, and I believe still intends, to publish by subscription, containing an account of this remarkable boy; and I therefore do not wish to enter into further particulars in this place, lest his father should imagine it to be injurious to the interests of that publication. But if that plan should be ultimately given up, I may at some future time furnish you with what I have been able to ascertain on this subject; not that I conceive any one of his methods can be employed to any useful purpose; they are peculiarly his own, and can only be advantageously employed by himself; but as a subject of curiosity they may be acceptable to some of your readers.

I am, sirs, yours, &c.

To Messrs. Nicholson and Tilloch. PETER BARLOW.

Plymouth, Feb. 8, 1814.

SIRS,—EVERY attempt to simplify and abridge calculation deserves attention, and I cannot therefore but view the question proposed by Sir H. Englefield, in your number for January, with particular pleasure and satisfaction.

In order to resolve it, I would first remark, that when the unit's place in any root is 0, 1, 2, 3, 4, 5, 6, 7, 8, 9,
the unit's place in the cube will be 0, 1, 8, 7, 4, 5, 6, 3, 2, 9.

This table, of course, can be acquired in a few minutes by memory. To exemplify its use, we may briefly state, that when the unit's place in any cube is 8, the unit's place in the root will be 2; or when the unit's figure in the cube is 3, the unit's place in the root will be 7.

Moreover it is a well known law existing amongst cube numbers, that if any cube number be divided into periods of three figures commencing with the unit, the number of these periods will show the number of figures in the root, which in the case before us never can exceed two; that is, the root of every cube number under a million consists but of two figures, the first of which, or that in the unit's place, being readly obtained by the application of the above table, there remains only to discover the other figure, which is the *cube root* of the *greatest cube number* contained in the first period. An

An example or two will make this clear. Let the cube number whose root is required be 804357. Here the first figure of the root will be 3, and the root of the greatest cube contained in 804 is 9. Hence the root required is 93.

Again. Required the cube root of 74088. Here 2 is the first figure of the root, and the root of the greatest cube contained in 74 is 4; hence 42 is the root required.

If only the figure of the unit, the two first figures, and the number of places be given, as stated in the question, the operation will be precisely the same. For by taking the example given in the question, the first figure of the root is 6; and as the number consists of 6 places, it remains only to find the root of the greatest cube number contained in 430, which is 7. Hence the root is 76. It may be proper however to remark, that this mode of stating the question fails when the given cube number terminates with a cypher.

Concerning the performing this "instantly and without any aid of writing," I beg to observe, that when I first discovered the principle, I explained it to two or three young gentlemen, pupils of mine, about twelve years of age, and who, after practising it for the short time of half an hour on a slate, would tell without the aid of writing, the root of any cube number under a million, instantly, and without the smallest hesitation.

The method applies also to any cube number above a million, when it terminates in a cypher; but I have not been able to extend it to cube numbers in general. I make no doubt but that with a little pains many other arithmetical operations might be simplified; and it would perhaps explain in a way not the most unsatisfactory, the truly astonishing powers of the "wonderful American boy." I am, &c.

George Harvey.

To Messrs. Nicholson and Tilloch.

XXIV. *Observations on the Doctrines of definite Proportions in Chemical Affinity.* By William Crane, Jun. M.D. of Boston, Lincolnshire*.

It is often a pleasing task to view the progressive improvement that is attendant upon different departments of science; in doing which we sometimes meet with hypotheses that were considered at first as crude, ridiculous, and soon almost forgotten, again revived, more clearly illustrated, and not unfrequently advanced as entirely new. Among many others that have been proposed in the science of chemistry, that of bodies uniting in definite proportions to form chemical compounds holds a distinguished place.

* Communicated by the author.

Mr. Higgins, in his Comparative View of the Phlogistic and Antiphlogistic Theories, conceiving that bodies were composed of minute atoms, was led, from the well-known fact that oxygen and hydrogen gases united in the proportion of 1 to 2 in volume to form water, and as they unite in no other proportion, to consider them as uniting atom to atom. "We must suppose," he observes, "that they contain an equal number of divisions, and that the difference of their specific gravity depends chiefly on the size of their ultimate particles; or we must suppose that the ultimate particles of light inflammable air require 2 or 3 or more of dephlogisticated air to saturate them." The latter, he infers, is not the case, from its being impossible to form such an intermediate combination as can be effected in the union of sulphur and oxygen. From this also he concludes, that oxygen and hydrogen "are incapable of uniting to a third particle of either of their constituent principles." Sulphuric acid, according to him, is formed of 1 particle of sulphur and 2 of oxygen, and sulphureous acid by 1 of sulphur and 1 of oxygen.

These speculations, owing to the imperfect state of chemical analysis, were overlooked and rejected by succeeding chemists, and little or no notice taken of them until Mr. Dalton, in a work displaying the vast powers of an enlightened mind, aided by industrious research, reproposed this doctrine, and again turned the attention of chemists to this important subject; and it appears to me, from perusing Mr. Dalton's new System of Chemical Philosophy, that he is unacquainted with the writings of Mr. Higgins on this subject. The former has, in the work just mentioned, stated that chemical attraction is guided by determinate laws; that bodies uniting to each other in one proportion only, unite atom to atom, or those which combine in various proportions form binary, ternary, &c. combinations, one atom of the one element being united to 1, 2, 3, &c. atoms of another. Examples are found in the union of oxygen and hydrogen, oxygen and azote, &c. one atom of the one uniting to one or more atoms of the other; according to some fixed mathematical relation, which Mr. Dalton apparently considers to be in arithmetical progression. In this view of affinity, it is of importance to determine the number of elementary atoms that enter into the formation of a compound atom. Hence, should this supposition be found correct, the errors arising from chemical analysis may be regulated by the aid of mathematical calculation. The two substances that have claimed particular attention, by which this may be accomplished, are water and ammonia, for the elements of these have as yet been found to unite in one proportion only.

Mr. Murray, in his Chemistry, has observed, that the facility with which the numbers can be assumed is one of the means by which we may be deceived. Dr.

Dr. Thomson, in his System of Chemistry, gave an outline of Mr. Dalton's theory, before the latter had himself made it public, and in his paper upon the oxalic acid in the Philosophical Transactions for 1807 has shown a very great coincidence in the composition of this acid, by calculation, with the result of actual analysis. In the 4th section of this paper he states that "it has been ascertained by decisive experiment, that elementary bodies always enter into combinations in determinate proportions, which may be represented by numbers;" and he finds an atom of oxygen to be 6, if an atom of hydrogen be estimated as 1. The Doctor has taken this estimate by supposing one atom of oxygen to unite to one of hydrogen, to form water; estimating the weight of hydrogen to be $14\frac{1}{3}$ and oxygen to be $85\frac{2}{3}$, to form 100 parts of water: hence an atom of hydrogen being to one of oxygen in the same ratio, it gives when reduced 1 to 6·48, and the Doctor rejected the decimals. Mr. Dalton takes his estimate from the proportion given by Humboldt and Gay-Lussac. It appears from their experiments to be 87·4 of oxygen to 12·6 of hydrogen; and the number representing oxygen will be 6·96, which approximates very nearly to 7, the number chosen by Mr. Dalton.

Sir H. Davy estimates the specific gravity of hydrogen to oxygen as 1 to 15; and as it takes 2 measures of hydrogen to 1 of oxygen in the formation of water, the ratio of the hydrogen to oxygen is as 2 to 15 *; "and it may be regarded as composed of 2 proportions of hydrogen and 1 of oxygen, and the number representing hydrogen will be 1, and that representing oxygen 15." This value is not taken by considering the number of atoms in the gases, a doctrine truly hypothetical, but from the proportions in which they unite, and their specific gravity. In considering the doctrine of proportion, as Sir H. Davy has observed, we certainly have no reason to suppose bodies composed of atoms that are indivisible uniting one to one, &c. In the present state of our knowledge this hypothesis of atoms cannot with propriety be admitted, as we have no evidence upon which we can be certain of the weight or number of atoms in any compound. "Our numerical expressions," as the author last quoted justly says, "ought only to relate to the result of our experiments."

It is not a little perplexing to observe Mr. Dalton, in his work, found his whole calculation upon the combination of oxygen and hydrogen as uniting atom to atom to form water; and in his chapter upon the combination of these gases, to find him declare that "after all it must be allowed to be possible that *water* may be a *ternary compound*." This conclusion appears in a great measure to be drawn from his experiments upon muriatic and

* Elements of Chemical Philosophy, p. 112.

fluoric acids, in which he concluded they were formed by the union of oxygen and hydrogen in different proportions. Later discoveries have proved this not to be the case. For oxymuriatic acid, now called chlorine, appears to be the basis of the muriatic acid; and fluoric acid, from the experiments of Davy, Gay-Lussac and Thenard, also has a peculiar base.

Notwithstanding the theory of atoms is so very hypothetical, it is applicable to calculation, as the numbers are relative, and taken from a fixed ratio. Therefore, if we take the original proposition that water be composed of equal atoms of these gases, I am inclined to think 6·96 as very near the true number for oxygen. I am not fond of rejecting decimals, as these, when there is a combination consisting of some high multiple, will lead to considerable error in the calculation, or when the substance under examination consists of many parts.

Dr. Thomson in his paper on oxalic acid, when calculating the composition of this acid, takes oxygen at 6, and hydrogen at 1, and carbon, with Mr. Dalton, at 4·3; and he finds from experiment the composition of oxalic acid to be,

Oxygen ..	64·69,	and by calculation	Oxygen ..	61
Carbon ..	31·78	.. ..	Carbon ..	34
Hydrogen	3·53	.. ..	Hydrogen	5
	100·00			100

If the number representing oxygen be taken, as I have above mentioned, 6·96, calculation gives

Oxygen	64·24	which comes extremely near.
Carbon	31·15	
Hydrogen	4·61	
	100·00	

The numbers given by Sir H. Davy, being reduced to Mr. Dalton's views, and applying them as above, appear to be too high. After Dr. Thomson, we find that Dr. Wollaston also read a paper to the Royal Society, giving an account of some experiments he had made upon sub-acid and super-acid salts. These he considers as particular examples of Mr. Dalton's more general rule, which he states thus: "that in all cases the simple elements of bodies are disposed to unite atom to atom singly; or, if either is in excess, it exceeds by a ratio to be expressed by some simple multiple of the number of its atoms."

When examining the oxalates, he found the super-oxalate to contain twice the quantity of acid the neutral salt contains. Hence it is probable that it unites in the proportion of 1 of potassa to 1 of acid, and 2 of acid to 1 of potassa: therefore the next combination, if it be formed, ought to consist of 3 of acid to 1 of alkali. To determine this, the Doctor took 400 grains of potassa and neutralized it with 30 of acid; to this he added 60 grains

more

more of acid; so there were 2 parts of potassa 24 grains each, and six equivalent parts of acid 15 grains each; which ought, as above stated, to form a salt of 3 of acid to 1 of alkali. But upon crystallization he found 2 salts, the one a binoxalate, the other a quadroxalate, or one of alkali to 4 of acid.

To make this answer to the theory of Dalton, the Doctor supposes the neutral salt to consist of 2 of alkali to 1 of acid, and the next 2 to 2, and the third 1 to 2, or 2 of alkali to 4 of acid. This is altogether hypothetical; and, as Dr. Wollaston observes, it is far from satisfactory that the alkali should be in excess in the neutral salt.

Although this appears against Mr. Dalton's theory, it is equally so to the law of Berthollet, that the combinations of bodies are as their relative attractions and acting masses: for, Why in that case are the salts above mentioned formed? The alkali ought to be diffused equally, and a salt consisting of 3 to 1 obtained.

Dr. Wollaston thinks it is probable that other ratios will be found, arising from the shape and polarity of the atoms, than the simple arithmetical.

Gay-Lussac has attempted to form a theory departing from the strict principles of Berthollet's views of affinity and those of Dalton. He thinks chemical affinity may be indefinitely exerted amongst the particles of matter, and compounds may be formed of variable proportions; but that insolubility, cohesion, and elasticity have a tendency to produce fixed combinations; and also that chemical action takes place with energy when the elements are in simple ratios, or some multiple of these ratios, by which compounds are produced that can be easily insulated.

By this means he endeavours to explain the phænomena attending chemical affinity; that bodies can unite in determinate proportions, and that under certain circumstances they unite according to the quantity of attracting matter, and form compounds of variable relations: and a view of this kind has been in some measure adopted by Berthollet. With respect to the combinations of the gases, Gay-Lussac has observed two laws. 1. That they combine in proportions having simple ratios according to their volume. 2. That gases by combination appear to suffer a contraction in volume in a simple ratio to the gas added. After having stated many examples of his first law, he also adds, that between the elements of the first combination there is no simple ratio; but this takes place in the second combination, when the new proportion will be a multiple of that first added. This, as Mr. Murray observes in his System of Chemistry, from which I have taken this outline of Gay-Lussac's theory, distinguishes his from that proposed by Dalton. To these speculations of Gay-Lussac, Mr. Dalton has replied in the Ap-

pendix to the Second Part of his work, appealing to his own experiments and those of the most celebrated chemists, that gases do not unite in measures proportionate to their volume.

After having cited particular examples that many of them do not unite in volume according to any determinate proportion, he concludes, "that gases in *no* instance combine in equal or exact measure." This, Mr. Murray thinks, might be perhaps properly extended to the whole doctrine of definite proportions.

Sir H. Davy, in his Elements of Chemical Philosophy, advances an immense number of proofs that the theory of definite proportions is not founded upon hypothetical reasoning. He, as is already observed, rejects the doctrine of atoms, and founds his calculations on the result of actual experiment. By this, one of the very great objections to Mr. Higgins's and Mr. Dalton's theory is removed; namely, that substances uniting in one proportion only form a binary compound, for we have no grounds upon which we can form such an opinion in the language of Mr. Dalton; it is merely an assumption. He supports, contrary to what Mr. Dalton has said, the theory advanced by Gay-Lussac, respecting the union of gases; which he beautifully illustrates by the combinations that are formed by azote and oxygen, carbon and oxygen, &c. And Mr. J. Davy, in his paper published in the Philosophical Transactions, On the gaseous Compounds of Carbonic Oxide and Chlorine, has given some very decisive examples. He observes, "the proportions in which bodies unite appear to be determined by fixed laws, which are exemplified in a variety of instances, and particularly in the present compounds. Oxygen combines with twice its volume of hydrogen, and twice its volume of carbonic oxide, to form water and carbonic acid; and with double its volume of chlorine to form euchlorine; and chlorine reciprocally requires its own volume of hydrogen and its own volume of carbonic oxide to form muriatic acid and the new gas."

Sir H. Davy in his work has noticed several of the experiments upon which Berthollet founded his views of affinity, and has pointed out the source of some errors. According to Berthollet's views, precipitates could not be thrown down in a state of purity, but must retain a portion of the substance with which the precipitate was previously combined: of this we have many examples to the contrary, as when one metal in solution is precipitated by another; and in the precipitation of magnesia, &c.

Sir H. Davy also observes, "that there is no difficulty in reconciling the doctrine of proportions with the influence of quantity. None of the experiments of M. Berthollet can be considered as strictly contrary to the doctrine, and some of the most important results of this sagacious chemist afford its confirmation."

From the above quotations and observations, I think it will clearly

clearly appear that the doctrine of Messrs. Higgins and Dalton with regard to atoms is truly hypothetical, and cannot with propriety be at present admitted: objections to this theory have also been pointed out by Dr. Bostock in Nicholson's Journal. The views I am most inclined to adopt are those of Sir H. Davy, as in these we are guided by the result of experiment and fact. The more accurately our researches are made, the more forcibly is the doctrine of definite proportions demonstrated. The researches of Mr. J. Davy on the combinations of different metals and chlorine, published in the Philosophical Transactions for 1812, and the late interesting publication of Berzelius, sufficiently prove the truth of what is above stated: although not standing the test of a rigorous mathematical examination, they are sufficiently near, considering that chemical analysis is still imperfect, to point out this grand law. Objections may undoubtedly still be urged against it, and that of solution has been considered by some as the most forcible; since, in this, bodies within certain limits appear to unite in any proportion. But surely there is some difference between dissolution and combination: in the former we find merely the cohesion of the compound aggregates amongst each other disunited, and the particles of the compound diffused throughout the fluid: in the latter, a decomposition takes place amongst the elements of which a compound is formed, it assumes entirely new properties; and it is here we are to look for the determinate action of affinity. If it were not guided by such a law, our results would always be showing different productions, and compounds consisting of the some proportion of elements would scarcely ever be obtained.

XXV. *Notes and Observations on the remaining part of the Seventh Chapter of Mr.* ROBERT BAKEWELL'S "*Introduction to Geology;*"—*embracing incidentally, several new Points of Geological Investigation and Theory. By Mr.* JOHN FAREY *Sen., Mineral Surveyor.*

[Continued from p. 34.]

Notes, &c.

P. 182, l. 6, from the ocean†.—† The great *south-eastern Denudation of England* ¶, probably includes the Basin of Paris in its upper eastern edge, P. M. xxxv. p. 130 and 134, notes; but

¶ The acknowledgement of this characteristic phænomenon of the counties of Kent, Sussex, Surrey, and the eastern part of Hampshire (the first Denudated tract that was well ascertained probably) seems with difficulty or reluctance made, by the Wernerian School: a learned Doctor, when lately reviewing

[P.182] but when the actual elevations of the different parts of the edges of the *Chalk* around this great denudation, are considered, it will be seen, that *a Lake*, in any great degree

reviewing the progress in 1813, of his favourite *Geognosy* (Ann. of Phil. iii. 29), ascribes to Mr. Thomas Webster (but perhaps rather erroneously?) the assertion, that the Parisian "new series of *flœtz Rocks*, which appear to *lie over the Chalk*," "constitute almost *the whole of the south-east corner of England*." Notwithstanding, that all the south-eastern coast between Folkstone and Eastborne (a direct distance of 45 Miles or more, shows Strata that *lie under the Chalk*, and that the same thing occurs thence inland, as far as Farnham and Petersfield, (distances of 85 and 5· miles respectively), within which large *denudated tract*, it is impracticable to find *any of the Parisian or London Strata*, or even the Chalk on which they should lie: but a quite different series of strata appear, dipping every where towards and passing under the nearest Chalk ranges.

Another learned Doctor, who claims, I believe, to be still "better educated," in the Wernerian subtilities, thinking perhaps (with an Edinburgh Professor, and Mr. B. P. M. xlii. p. 167) that the *Section* previously made across this district, formed principally on "Stage-coach observations," was utterly beneath the dignity of his *Geognosy*, and unworthy even of having *its most obvious facts* as above mentioned, attended to, or examined; when he returned two or three years ago from a southern *Geognostic Tour*, presented an account (as Mr. Webster is stated by Dr. T. to have done), representing, as I was informed, the strata *in this denudated tract*, whose places are really *far below the Chalk Strata* of its edges, as *lieing above the Chalk!!*. It was however then thought proper to strike *this part* of the Doctor's discoveries from his paper, before it was inserted in the Geo. Tran. vol. i.

A very striking feature of the south-eastern Denudation of England, and which my manuscript Section made in 1806, mentioned P M xlii. p. 167, was in a principal degree intended to show, is the *rise* of the strata from the vale of the Thames at London, southward, to a *strata-ridge* line, ranging nearly E and W, which crosses the London and Brighton Road at Hand-cross 35 Miles from London, and the contrary *dip* of the strata from this place to the Sea at Brighton; and also, that from the Thames the strata begin to rise again through Islington, from a *strata-trough* line, ranging along the vale of the Thames. In your xxxixth vol p. 27: Note, I have mentioned, that some distance beyond Brighton, the strata rise again southward, from another *strata-trough* line, which ranges thence between the Isle of Wight and Portsmouth, and proceeds westward into Dorsetshire. Which last Trough in the English strata, I investigated to a certain degree, near Portsmouth, and made a Section, of which I circulated several manuscript copies in the spring of 1812; which showed, that the notion so often published, of the Isle of Wight having been *forcibly rent off* from Hampshire, and the channel between them occasioned by the fissure or chasm thereby opened, was entirely without foundation, and that the Isle of Wight, was on the contrary, a hummock or part remaining, of the ridge in the strata, south of the trough above mentioned; and that instead of a chasm, *the highest of the known British strata*, in unbroken continuity, occupies the bed of the Channel and adjoining coasts of the Isle of Wight and Hampshire, as my other Section mentioned, had done with respect to the Thames Trough at London In your last volume, p. 395, Mr. Webster appears to have given the names of "Isle of Wight Basin," and "London Basin," to these two Troughs in the strata, that I had previously ascertained, as above mentioned.

larger

[P.182] larger than that part of the channel of the Sea which now occupies the denudation, (and rests partly on *under-strata to the Chalk*) is a very improbable supposition (see my Note on p. 185); and Mr. B. must not avail himself, in this case, of the existence of a vast series of strata *on the Chalk*, which supplied edges, (since "broken down" by the Channel) to retain the waters of his Lake, since *these upper strata*, are the very things *to be accounted for*, and for the formation of which alone, were the *Lakes* of the Paris Gentlemen and himself, invented; although "the assumption of imaginary facts in Geology, has a tendency to retard the progress of the science, more than any other cause." Mr. B. P. M. xl. p. 47.

183, l. 19, subsidence of mud*.—* Mud will not form *strata*, except perhaps of alluvial Clay or loam, but more probably, "*les marnes argilleuses noires*," p. 336.

184, l. 16, osseous remains*.—* The fossil Bones of Gibraltar are found *in Caves* or fissures, partly filled with calcareous *Tufa*, near to the Tarfes, at the S end of the Gibraltar Rock, as I have been informed, see page 18, and M. Cuvier has stated.

185, l. 9 and 10, waters in the Baltic*.—* Here Mr. B. seems supposing, the northern edge of the Chalk, from Flamborough-head to Jutland, Zealand and Gothland, (P. M. xxxv. p. 131), to be entire, and to act as a partial or an entire dam, to a greater *Lake*, than that which I have objected to in my 2nd Note on p. 182, and here my objections will apply with greater force.

M. Meicrotto long preceded Mr. B. in the assumption of large *Lakes* in the north of Europe, see M. De Luc's Geo. Trav. in France, &c. i. 420.

l. 20 and 21, in Chalk differ †.—† The organic Remains, in a large proportion of nearly *all the strata* which contain them, *differ most obviously from each other*, Rep. i. 109; and I entertain no doubt, that when sufficiently minute and extended examinations and descriptions of the Reliquia are made (Mont. Mag. xxxiii. p. 514), that the *species* thereof will characterize the different *strata*, and even the different beds, in many strata, in a very satisfactory manner. Mr. Sowerby's work called "Mineral Conchology," of which five numbers are published (9 now), is deserving of every encouragement, towards this end, by those who have it in their power, to send him ample and perfect specimens of *fossil shells*, with their *exact localities*; and when it can be done, their *places in the strata also*: and when several sorts of shells are sent, to distinguish those which are found uppermost, and how far above each other in the strata, &c.

P. 186,

P. 186, l. 2 and 3, partial formations*.—* There seem no grounds for stating Chalk to be a *partial* formation, and very little, except the modern whims respecting *fresh-water Lakes* and Shells, for saying the same of any of its superincumbent *strata*, if the *imbedded masses* belonging to them, are properly distinguished, see my Notes on pages 168, 177, 182, &c.: *Gravel*, Mr. B. will remember, is always partial in its formation or distribution, and always uppermost.

187, l. 15, which protect them*.—* If vegetable *soil* was less liable to be acted on, and displaced by the elements than hard *rocks*, we might be more disposed to admit, that "the loftiest eminences" are by this means protected from being levelled;—where ever did *a whole mountain* suddenly fall down?, such are common-place, yet very unphilosophical assertions.

188, l. 5, deep ravines*.—* These and gullies, formed "in the sides" of mountains, are no proof that *their tops are lowering* or degrading, sensibly, by the action of the elements, any more than large blocks found at the foot of a mountain, prove the same thing; since such blocks will in general be found, on examination, either to have fallen from some cliff or steep place, near at hand, or to have been moved from some distant stratum; or, if much *rounded*, perhaps from the antipodes, as likely so, as from *the top* of any adjoining mountain, (as seems here assumed), and often, far more likely I think, when the completely different nature of the rounded blocks and the adjacent strata, is taken into the account.

l. 19 and 20, blocks of white quartz *lie*†.—† If the violence of the waves of the Atlantic ocean, acting on the rocks and shores of the west of Scotland, is unable to *throw down* the veins of Basalt, which there stand up like "immense walls of stone," when the surrounding rocks are decomposed and *torn away*, p. 210; how has it happened, that harder and more compact veins, of white quartz, intersecting slate, on Beacon and other hills, in Charnwood Forest, are found levelled, and that blocks of it "*lie* upon their summits?:" and how, if quartz veins were in any degree as numerous in these Rocks, as for a moment to be supposed, to have furnished the millions of rounded "white quartz pebbles spread over the midland counties," has it happened, that dykes of quartz, nowhere appear *above the surface* in all this forest?; but on the contrary, that a smooth surface of Red Marl prevails, over all except a comparatively small part, occupied by Slate or Sienite peaks, or cliffs

[P, 188] cliffs of such, Rep. i. 182: and the quartz veins observable *in the cliffs*, are but very few, see p. 287. In short, *theory* seems here, and at top of page 191, wildly let loose, in search of "imaginary" causes, and effects also: see my Note on p. 285.

Mr. Allan, I observe, p. 192, of your last volume, considers the white Quartz masses on the surface of some parts of Cornwall, as derived from the decomposition of Veins, in Killas strata, which once existed *on these spots*. I beg to invite your Readers who may have the opportunity, to investigate the validity of this inference.

190. l. 15, an easy explanation †.—† The irregular sand pipes from Drigg in Cumberland, P. M. xl. p. 390, seem more evidently referable to *Lightning*, than any other Geological phænomenon that I have seen, and are no where noticed in Mr. B's volume, I believe:—when they first came into notice here, I was shown one of these Pipes (at No. 3, on the north side of Lincoln's-Inn fields) as an organized remain, petrified *Wood*, some said:—but I was not for an instant deceived. What relation is there between these Sand Pipes, and the *Osteocolla*, mentioned under that article in Dr. Rees's Cyclopædia?, from No. 39, of the Phil. Trans. &c.

192, l. 9, new mountains*.—* It in no degree appears, that the fanciful, rather than the *grand* "revolutions of the Globe," here contended for, are more necessary "in the œconomy of nature," than they are real;—Mountains more than Continents, do not grow old, and become less fit for the uses for which their bountiful Creator intended them, but the reverse of this: and though all *the living organized Beings*, which, in such myriads, people its surface, and have their different, but very limited periods of existence, and then their visible and material parts quickly loose their bond of connexion, and are fitted to form parts of other living Beings, in most cases, yet *the Mass of the Earth*, seems wisely fitted, to continue the theatre of these grand organic revolutions, with increasing happiness to the ever changing individuals (of the Human race in particular) to the most indefinite periods of time, if it should so please its almighty Author; and as we may perhaps reasonably conclude will be the case, from the recently demonstrated *facts of Astronomy*, from whence similar whims, regarding inherent causes of destruction and end, have been banished.

Although we can see no necessary cause of *end* to the duration of *the Earth*, we can plainly, by means of its imbedded organic remains in particular, perceive that it had *a beginning*, and reached its present state, by progressive and very

[P.192] very slow steps; patient investigation of the facts, and just reasonings upon them, such as the physical astronomers have persevering applied, in a century which is past, will certainly be rewarded with results in this science, of no ordinary interest to our Race, I venture to think, see my note on p. 328.

199, l. 6, unfavourable to vegetation*.—*Rep. ii. 185, and 447.

l. 25, the same purpose †.—† Rep. ii. 107, 409, and 412.

200, l. 19, worn by attrition*.—* Between the high mountains at the eastern end of the Caledonian Canal in Invernessshire, the largest quantity of alluvial Clay seems lodged, interspersed with a few huge Bolders, which I have perhaps any where seen¶ (see my Note on page 52): to me it seems probable, after conversing with Mr. Thomas Telford on the subject, as to the facts of these and other parts that I had not seen, that the immensely deep *Lakes* in the line of this Canal, may be parts of a valley or chasm, as deep as these Lakes, which once opened into the German Ocean, and that enough of this alluvium has since been lodged, not only to fill up the eastern end of this valley, to its present level, but even 100 or 150 feet higher, until *a subsequent excavation* therein, took place, and left a tablet of this Clay, skirting the mountains, to nearly the same level, for great distances, as monuments of its state at one period. The vast

¶ The alluvial *chalky* Clays of England, which I have mentioned, in Bedfordshire and other places, see my Note on p. 52, appear very liable to be mistaken by, and to mislead observers, who have not accurately studied the circumstances attending *stratification*, in the same school with the practical *sinkers* or the superintendants of Coal Pits, and other deep excavations (see my Note on p. 44); to which classes of Men the distinction is perfectly known, and evident (however difficult it may be to define the same, so as to instruct or even to draw the attention of *learned Geognosts*, to those distinctions): and *the alluvium of the surface*, whatever its thickness, or the technical or local Name by which it is distinguished (as Alluvium, Bareing, Callow, Clay [mixed], Clearing, Corn-soil, Cover, Corn-clay, Earth-cover, Gravel, Gingle, Keale, Loose-earth or soil, Ratchel, Ridding, Rock-cover, Rubbish, Rubble, Rummel, Sammel, Sand [mixed], Scale, Skerry, Soil, Ter, Till, Top-earth, &c. &c.) is perfectly known by them from *the regular strata* beneath these disturbed matters, of whatever kinds, or whatever Names such undisturbed matters may be known by, in the district (as Fast-country, earth, rock or soil, Measures, Metals, Mine, Regular-measures, Rock, Shelf, Strata, Undisturbed-ground, whole-ground, &c. &c.) And among these Men, the merest novice would be ashamed of making such blunders on this essential head, as have gravely been published to the world, as to red *Marl with Shells* in it, on Chellaston Hill, (see my Note on p. 176), *Chalk strata* in Huntingdonshire and Rutlandshire (note on p. 259), &c.

I strongly suspect, that what Mr. Webster in your last volume, page 325, calls *the Chalk Marl* (notwithstanding that *this name* was already appropriated

[P.200] vast beds of Till, S of the Ochills (Wernerian Trans. i. 481) appear also, other instances to show, that the skirts of Mountains and mountainous countries, are less commonly, strewed with the worn fragments of *their own Rocks*, than seems here by Mr. B. and is very generally, assumed, by Geological writers.

201, l. 18, by other processes*.—* Why may not there be vast *regular strata of Sand?* I have met with nothing to contradict such a conclusion; but much to confirm it, see my Note on p. 60.

202, l. 10, classed with *alluvial* products*. — * Very few Breccias or Conglomerates, will perhaps, come into this Class, see my Notes on pages 44 and 50. In Anglesea, I have lately had the opportunity, rather unexpectedly, of seeing a conglomerate Mass or Rock, such as I had not previously noticed, in examining, what I conceive to be the same series of Strata, in the Great Forest of Brecon, and other parts of the edges of the great South-Wales Coal-basin or *Trough*, and of those of the Forest of Dean and of the Clee-Hills.

This Anglesea conglomerate I first observed, at the SW corner of a very poor common, called Rhos-mirch, $\frac{3}{4}$*m.* NE of Llangefni Town, from which a new Road is about to be made, under Mr. Maughan, the Commissioner for the Inclosure, across this common. There is a quarry open in this Rock, from whence a Cottage has lately been built, just by it, and this stone is used also in several other Buildings SW.

priated to a stratum *below* the Chalk, Rep. i. 112), immediately *incumbent upon* Chalk, will turn out to be *alluvial Chalky Clay*, and *not any stratum*, underlieing the red mottled Clay, as is stated, at the top of page 396.

In p. 482, the description is given, of a patch of alluvial chalky clay, resting on a detached hummock of hard Chalk and green Sand, in the Chalk-marl district, near Foxton, in Cambridgeshire, " situated *to the west* of the great range of Chalk;" and yet the writer would persuade us, that the numerous pieces of worn Chalk and Flints in this Clay patch were lodged there, by " an ancient current, the course of which was *from West to East!*."

It may perhaps be doubted, whether this Gentleman is possessed of the means of knowing certainly, the organic remains *older than the Chalk*, from other reliquia? or, that he can show, that no such shelly Limestone Strata, and Rocks of Greenstone, as he alludes to, occur in Europe, *to the South-east* of Cambridgeshire?—on failing in which showings, Mr. Smith's and my multiplied observations, as to *the removal of known alluvia from SE to NW*, will stand unaffected, by this pretended case to the contrary. This is a subject, which I believe Mr. Bakewell has overlooked, see my Note on page 52.

The

[P. 202] The principal masses in this conglomerate are quartz grains of very various sizes, variously intermixed with masses of white Chert, of Jasper, of Slate, &c. &c. It forms regular beds, dipping easily SE, and passing under the very regular black Marble Rock (in which there is a considerable Quarry and Lime-Kilns called Cwaise-bangor, about ¼*m.* NNE) constituting the lower part of the Limestone floor or trough to the Coal-measures, mentioned in my note on p. 108. In general, where I had opportunities of examining this gritstone below the black Limestone Rock, it consisted of very variable coarse Grit-stone, but in other places, the same had a few masses of Jasper in it (as I had noticed in South-Wales), masses of Chert in others, and on the W side of Nant-hoorva Quarry, ⅓*m.* SW of Llangefni, the masses of Chert are large, resting on coarse Slate, and seem to graduate or pass into Jasper, as they do also on the SW side of Graig-fawr Rock ENE of the Town. I much wish to call the attention of Geological observers, to the careful examination of this and other variable Rocks, as presenting matter of important consideration, on the formation of strata: Mr. Jameson's late opinions and mine, seem very nearly to coincide, as to their cotemporary formation.

l. 18 and 19, more properly to the vegetable†.—† Rep. i. 311, Mont. Mag. xxxiii. p. 515.

205, l. 16, on this coast*.—* Without doubt the ground has sunk, comparatively at least, on all the British Coasts, see *Encroachment* of the Sea in Dr. Rees's Cyclopædia, my 1st Letter, vol. xlii. p. 58, and Note on p. 11.

206, l. 3, than the Geologist*.—* No true theory of the Earth or system of Geology will ever be produced, which does not embrace a knowledge of "alluvial ground," so universally spread, equally or more intimate, than with all the "primitive and transition" countries in the World; because alluvium, besides being vastly more spread, indicates *later operations* on our Planet, and more within the reach of our investigations, than the *formation* of Mountains; the force of this remark Mr. B. may comprehend, from my Notes on pages 16, 175, 200, &c.

l. 12 and 13, and explained the principles†.—† In 1784, Dr. *James Anderson* clearly *explained the general principles of draining Land* (in his Essays, i. 150), more of these principles, indeed, than Mr. Joseph Elkington knew, in 1794 (Rep. ii. 363, and P. M. xli. p. 215)? Years before either of which periods, they were *practically known* to Mr. *William Hart* (Rep. ii. 371) and others, engaged extensively in draining, for Proprietors and Occupiers of Land in

[P. 206] various parts of England. At length, after repeated attempts with Mr. E., had proved, that he could neither sufficiently explain, the already well known principles of Draining, or add any *new* one, as the too sanguine zeal of the President of the Board of Agriculture had imagined (see Communications to the Board, vol. i. pages lxi. and lxxvi.) and so persuaded the Legislature: in order to satisfy the condition annexed by the latter, to the *public Reward*, thus prematurely voted to Mr. E., the measure was adopted, of employing two or more young Men, to travel through the various districts in which Mr. E. had effected or attempted to make drainages, and from their own observations and study of the subject, and what they could draw from Mr. E. to furnish, in a degree at least, such a work on the subject, as the Legislature expected, as the result of the national liberality. Mr. *John Johnstone*, one of these young Men, very ably executed his task, and explained more fully than had previously been done, the principles and practices of this important art, in a Work prepared by him, but which is on almost all occasions (as in the present one by Mr. B. apparently) improperly *ascribed to Mr. Elkington*, Rep. ii. 372 *Note.*

[To be continued.]

XXVI. *Remarks on the geological Theory supported by* JAMES SMITHSON, *Esq. in his Paper on a saline Substance from Mount Vesuvius.* By J. A. DE LUC, *Esq. F.R.S. &c. &c.*

Windsor, January 1814.

SIRS,—IN the number of your Magazine for last December, which contains my paper addressed to you, "On a Phænomenon of Mount St. Michael in Cornwall," I have found a paper under the title: "On a saline Substance from Mount Vesuvius, by James Smithson, Esq." intended to support a new geological system, thus introduced at the beginning of the paper. "It has long appeared to me, that when the *earth* is considered with attention, *innumerable circumstances* are perceived, which cannot but lead to the conclusion, that it has been once in a state of *general conflagration*. The existence in the skies of planetary bodies, which seem actually *burning*, and the appearance of *original fire* on our globe, I have conceived to be mutually corroborative of each other; and at the same time when *no answer* could be given to the most essential objections to the hypothesis, the *mass of facts* in favour of it fully justified, I thought, the inference that our *habitation* is *an extinct comet or sun*."

Such is the system: and we must now consider what is that

that *mass of facts* that *justify the hypothesis*, of which it may be supposed the author adduces the most striking: he thus continues: "The mighty difficulties which formerly assailed this opinion, great modern discoveries have dissipated. Acquainted now, that the bases of alkalies and earths are metals eminently oxidable, we are no longer embarrassed either for the *pabulum* of the *inflammation*, or to account for the *production* of it. In the *primary strata* we behold the result of *combustion:* in them we see the *oxide* collected on the surface of the *calcined* mass, first *melted* by *heat*, then by its increase arresting further combination, and extinguishing the *fires* which had generated it, and in fine become *solid* and crystallized over the metallic ball. *Every thing tells* that a large body of *combustible matter* remains inclosed within the *stony envelope*, of which *volcanic eruptions* are partial and small ascensions. Under this point of view, a high interest attaches itself to *volcanoes* and their *ejections;* they cease to be local phænomena; they become *principal elements* in the *history of our globe;* they connect its present with its former condition."

This surely is as new, as it is a grand system; but what is its foundation? The author tells it himself in the title of his paper; it is on *a saline substance from Mount Vesuvius;* and he informs us that it was *sent to him from Naples, while he was at Florence in May* 1794, *with the request of ascertaining its nature;* after which he thus continues: "The general examination which I made of it, showed that it was what at that time was called *vitriolated tartar*, and it was in consequence mentioned as such in an Italian publication soon after."

Then he gives the following particulars concerning the manner in which that substance had been found: "I was informed *by a letter*, that it had *flowed out liquid* from a *small aperture* in the *cone of Vesuvius*, which I apprehend to have happened in 1792 or 1793."

Thus we may judge, that the author has never observed himself Mount Vesuvius, and that he knows only by report a few of the phænomena of that *volcano*. He then proceeds to detail the chemical analysis which he had made of that *saline substance;* but as he is aware that so small and even so indirect a fact could not support his *igneous system*, he ventures on another ground, in which I shall now follow him, and afterwards return to *volcanoes*.

"In support (he says) of the *igneous origin* here attributed to our *primitive strata*, I will observe that, not only *no crystal* imbedded in them, such as *quartz*, *garnet*, *tourmaline*, &c. has been seen *inclosing drops of water*, but that none of the materials of the *strata* contain *water* in any state."

The

The author appears not to be acquainted with such mountains formed of *primitive strata* as are those observed in the central ridge of the Alps, in which is found the *rock crystal*, of a *quartzeous nature*, and even the purest *quartz*; a fact directly contrary to his assertion: but the circumstances attending that *crystal*, and the *crystal* itself, indicate so incontestably its *aqueous origin* and that of the *strata* in which it is found, that I shall describe the most important of them.

At the time when I first travelled in the Alps, which was in 1744, *rock crystal* was employed, by cutting it, for various ornaments in houses, as lustres, sconces, and frames of various sorts, as may be seen in some ancient palaces.

Considering that the circumstances attending that *crystal* in the *strata* could lead to the natural history of the latter, and as living at Geneva near the Alps, my brother and myself resolved to visit the places whence that *crystal* came, and to converse with those on the spot whose profession it was to extract it, in order to be informed of the nature of their work.

After having travelled in many parts of the Alps, we generally found that *rock crystal* belonged to a certain class of *micaceous schistose* strata, with many circumstances attesting the *aqueous origin* of that *crystal*, and consequently of the *strata* themselves. The following was the information which we received.

A general fact is, that *rock crystal* is never found but in *cavities*, the sides of which all around are covered with its *prisms*. Those of the inhabitants of the Alps who made that *crystal* the object of their pursuits, commonly called *crystal-hunters*, knowing by the aspect of the *strata* those in which such *cavities* were likely to be found, and that they could not be discovered but in *abrupt sides* of the *rocks*, because the rubbish and grass cover the other parts, used a very hazardous method to find out those *cavities*. On the top of such abrupt sides in which they discovered their characters, they bored some holes in the solid rock at different places, for the following purpose. They fixed very solidly poles in these holes, and twisted round them a rope, at the one end of which hung a sort of basket: in this the most courageous of them was let down, allowing him a greater share of the adventure; while his associates, to whom he trusted his life, held the other end of the rope at the top.

As that man went down along the different parts of the rock, he struck it with a hammer, and by the different sound of the strokes he judged whether the rock was hollow. If there was no sensible difference in the sound, he did not think it worth while to open the *cavity*; but if the sound gave him the hope of finding a large *cavity*, then care was taken to secure him better, as he was to open it, and, entering it, to detach the *prisms*.

Fragments being only used for the common purposes, the man who entered these cavities broke with his hammer all the projecting *prisms* around it; and the quantity was sometimes so great, that the associates made much money from what they obtained in one of them. Such are the general circumstances concerning *rock crystal* found in that kind of *schistose strata;* but the particulars to which I now come, are no less important to the natural history of that mineral.

I shall mention as a first important circumstance, the *size* of some *prisms* which have been found. I have known two *prisms*, one of which was perfectly transparent, employed in a most beautiful work for the late Empress Queen, which *prism* was from 9 to 10 inches in diameter: the other, less transparent, was above one foot. I think I can now say with more certainty than the author has done for his hypothesis, that *such crystals have never been seen as the product of fusion.* But the character of true conclusions from the phænomena, is manifested by the coincidence of circumstances in the same object; and it is the case in the system of the *aqueous origin* of our *mineral strata:* a confirmation to which I now come.

As the men who followed this pursuit knew also that some curious travellers in the Alps paid them for the trouble of detaching parts of the crust covering the sides of the *cavities* with its *prisms*,—which crust, by naturalists of many countries, is called *druses*,—they carefully detached them; and we had the opportunity of purchasing many which are in our collection of minerals remaining at Geneva. Now, in these *druses*, many proofs are found that they were formed in *cavities* during the time that our continents constituted the *bed of the sea*, and that consequently these *cavities* were filled with *water*. That great geological fact, so contrary to the *igneous system* of the author, is demonstrated by the immense quantity of *marine exuviæ* found in the *strata* commonly called *secondary*.

By the inspection of the *druses* that we have in our collection, it is impossible not to be convinced that the *prisms* have been formed, not only in *cavities* filled with *water*, but by a process in which each *prism* increased *successively* in *size* by new additions; and that they were as *shooting* at the same time in different directions from the same points. In one of these *druses* a large *prism*, in its increase of size, has enveloped a small one; they are both very transparent; and this phænomenon might be overlooked, did not the *small prism* project on one side of the other: but this circumstance directing the sight in the large *prism*, the small one is seen in it by the different refraction of the light. In another druse, by some change in the *successive* process of crystallization, a large *prism* has divided itself into small ones, like the branches of a tree. It

It would be endless to describe further the phænomena of *druses*, all leading to the same conclusion : the various manners in which the *prisms* cross each other, divide and unite again. But there is a particular circumstance, which is exemplified in a process that we see going on, that of the *crystallization* of *salts* in *liquids*. Each *salt* has its particular character and shape; and if a *liquid* hold in solution different *salts*, they crystallize separately in known circumstances, especially in different temperatures. Now there is a similar phænomenon in the *crystallization* of *rock crystal*, showing that in some of the *cavities* in which it has been formed, the liquid contained in solution other mineral substances, one in particular, whence have proceeded small crystals of *green shorl*, from which many *prisms* of *rock crystal* are made, unfit for any other use than that of variety in mineral collections.

These *green shorls* were at first supposed to be *mosses* that had grown in these cavities; a supposition contradicted by its being impossible that *land vegetables* should grow in the sea; but by observing these *shorls* with a magnifying glass, the mistake is soon discovered, by the *angular* form of their thread. We have a *prism* of rock crystal made almost opake by these *shorls*, in which one of them having been destroyed, has left an angular hole from one side to the other, through which a horse-hair can pass.

I now come to the assertion of the author, *that none of the crystals has ever been seen inclosing drops of water*. This might be, without affording any argument against the *aqueous* origin, and for instance, of *rock crystal;* for there is no reason *à priori*, that water should be inclosed in any of its *prisms*. However, the fact itself contradicts his assertion. It is natural to infer that he has not seen the case himself, since he never alludes to *rock crystal*, though so important to the natural history of the *primitive strata*. But I have seen many *prisms* of that crystal containing *water*, and there is one in our collection, the particulars of which I shall describe.

This prism is about two inches in diameter; it is traversed by many small crystals of *green shorl*, between which a small quantity of *water* remained engaged during the increase of its size. That *water* is manifested by a *bubble of air*, which remained inclosed with the *water* retained also by the *shorls*. By inclining the *prism*, that *bubble* moves gradually from nearly one end to the other of the prism; but as, according to the *shorls* that it meets in its way, its direction is determined, it does not follow the same course in every trial, except when the inclination of the *prism* happens to be the same and on the same side, and it shows thus the space which is occupied by the *inclosed water*.

 Now,

Now, since the author says in favour of his *igneous system*, that *no crystal such as quartz has ever been seen inclosing drops of water*, which ought to be the case in that system; the contrary being the *fact*, is a peremptory proof against it.

I now come to the inferences which the author derives from the phænomena of *volcanoes*, making however this previous remark, that in this case again he has made himself no observations, nor even on Vesuvius, of which alone he speaks; for he says that he had received his information from a correspondent at Naples. But besides, the only fact on which he has built his whole system, is that *saline substance*, of which even the particulars that he relates are either not well expressed, or must proceed from a very inaccurate observer. I shall prove hereafter that this account is erroneous; but I must first state the nature of volcanoes, on which all their phænomena depend.

What are *volcanic eminences?* In what manner do they begin to be produced? How do they increase in height and extent? Fortunately the answer to these questions is obtained from a known event, transmitted to us by witnesses of the whole process, from beginning to end, in the formation of a new *volcanic eminence* between Naples and Puzzoli, which has retained the name of *Monte Nuovo* (new mount). I have given the whole account of the event from those witnesses, in p. 398 of the xith volume of my work, *Histoire de la Terre et de l'Homme*, of which account the following is a translation.

"In the night between the 29th and the 30th of September 1538, after two years of almost continual *earthquakes*, and especially after a day when the earth had been continually agitated, at last an *opening* was produced in a very fertile land; through which *opening* came out so much fire, and such a quantity of stones and cinders, that they produced a small mountain, now called *Monte Nuovo*; it destroyed many houses, fields, men and cattle, and buried under its materials the borough of Ripercola: it is about three miles in circumference, and its height is not much less than that of Mount Barbaro near it, which is estimated at a thousand steps; the ejections from that opening extended down to the sea. At the summit of this *mount* is still seen the mouth which ejected such a quantity of matter; which ejection however ceased very soon. The *Lago lucrino*, thus called from the *lucre* or profit resulting from an abundant fishery, was so much filled up by the matters thrown out of that opening, that it is now only a marsh covered with reeds."

From this known and well circumstantiated event, we are enabled to answer the above important questions concerning the formation of *volcanic cones*. A first explosion produces an *opening* in the *mineral strata* of the land; through that *opening* come

come out successively *lavas* and *showers of cinders*, which by degrees produce an accumulation in the form of a *cone*; and the materials of such *mounts* are as distinct from the *original* strata, as iron is from wood.

The author has no real knowledge of the various *ejections* of Mount Vesuvius, since he derives his hypothesis from the specimen of a *saline substance* sent to him *from Naples*. But to conclude a system on that *specimen*, is the same as to decide the nature of all the *mineral strata* of an unknown country, from a *fragment of stone*. If he had known my geological work above mentioned, *Histoire de la Terre et de l'Homme*, he would have found in it, from the observations of my brother, such an exact description of all the phænomena of volcanoes, as must have superseded in his mind the erroneous information which he appears to have received from Naples.

Nobody, to my knowledge, has observed the various volcanic phænomena more thoroughly than my brother, not only of Vesuvius, but of Etna, and of the *volcanic* islands of Lipari, near Sicily. He has seen actual eruptions of Vesuvius, and followed the steps of new *lavas* flowing over old *lavas*. This progress continues as long as the new *lava* retains a sufficient heat to maintain its softness; but at a greater or smaller distance, though still *red hot*, it hardens and breaks; then the broken pieces, still propelled by the soft *lava*, rise in heaps at the extremity, and tumbling over one another, they produce a particular noise heard at a considerable distance. Such are the real characters of *lavas*, unknown to the author, who has never had the opportunity of observing them himself.

My brother has been no less attentive to the *saline* and *sulphureous* substances observed on the sides of Vesuvius. The author says he was informed *by a letter*, that the *saline substance* of which he had received a specimen, *had flowed liquid from a small aperture in the cone of Vesuvius*. I think he might have judged himself, that this was impossible. How could so small a quantity of matter, susppposed to be in *igneous fusion*, preserve the *heat* that is supposed to make it *liquid*, while passing through a great thickness of old *lavas*, long reduced to the temperature of the air? This supposition, therefore, is absolutely improbable. My brother, who has carefully observed those substances that appear on the surface of the old *lavas* towards the top of Vesuvius, and has detached fragments of them preserved in our collection, has invariably found that they had been, and continued to be, produced in *crevices* which emit *fumes*: thus showing that these substances are a sort of *sublimation* which accumulates against the sides of the *crevices*, without any appearance of their having been ever in a *liquid* state.

With

With respect to the *nature* of *lavas*,—a most important point concerning all the volcanic phænomena, with which the author is totally unacquainted,—my brother has found in it all the characters of a *combustible* substance, which is *burning* before it comes out, and continuing to *burn* outside as long as it retains a sufficient heat. This combustion is shown by the *sulphureous smoke* that *lavas* emit all along their course. But that *smoke* is not the immediate product of the combustion; it proceeds from the decomposition of some *elastic fluids*, which, as long as they find no *vent*, are the cause of the ascent of *lavas* in the *cones*.

This circumstance is known to the inhabitants around Vesuvius; for, when they observe almost a cessation of the *smoke* issuing from its *crater*, they are in fearful expectation of the consequences, as it indicates that some *lava*, ascending in the channel, has cooled and hardened in it. The first consequence is *earthquakes;* and the next probable event, which is very dangerous, is the bursting of a *lava* from some other part of the cone. If that new lava flows over cultivated grounds, it sets on fire trees, vines, and even houses. But the new *lava* melting that which had stopped the channel, the *smoke* again issues from the *crater*, and these fatal effects cease.

The *showers of cinders* sometimes emitted from the craters of *volcanoes*, being an astonishing character of their operations, interested my brother very much, and he resolved to try whether it would be possible to observe it in the very *crater* of Vesuvius. At a time, therefore, when it was observed from Naples that such *showers* were frequently emitted, consisting of large *red-hot masses* mixed with smaller, always preceded by a *thundering* noise, and thrown up very high, which in the night appeared a most tremendous fire-work; my brother, taking notice of a favourable circumstance, that of a strong wind which repelled these ejections on one side of the *crater*, thought to avail himself of that opportunity. He therefore set out immediately from Naples, and ascended the *cone* on the side against which blew the wind.

Arrived at the top of the mount, he descended into the *crater*, and came as near the edge of the channel as he thought it prudent. At first he saw only some redness deep in the channel; but after a little time he heard the *thundering* noise; it began very deep; then it was heard to ascend at the same time that the *red matter* rose; and when its column arrived at a certain height, in a part of the channel which was wider, the *elastic fluid* that had pressed it so far upwards, burst through it, and a *shower* was produced. This being a remarkable phænomenon, not to be expected frequently with the favourable circumstance of a strong wind, my brother remained there a sufficient time to see it repeated with its various degrees. Desirous

Desirous to know also in what state was the matter thus thrown up very high, and falling in *showers*, he followed with the sight some of the largest masses; and observing in what part of the *crater* they fell, he hastened there before another explosion: he found that they were come out very soft; for the largest, preserving longer their heat in their way through the air, were flattened like *cakes;* but the smaller masses, though still red-hot, had preserved their various forms.

It is from these *ejections*, carried out of the circumference of the crater in different directions by strong winds, that are formed the *slopes of loose cinders* which make the ascent to the *crater* very difficult, because they slide under the feet: those, therefore, who are not used to climb on all kinds of mountains, are obliged to employ some men, ready there for that purpose. These men have a belt, with a loop of string fixed to it, taken hold of by the people who could not ascend without help, who are thus dragged up.

But my brother, used to climb the slopes of *rubbish* in the Alps, which oppose the same difficulty as those of *cinders*, judging that on the slopes of cinders might be found all the kinds of *ejections* from the *crater* of Vesuvius, walked over them at different times in different directions, with expectation that they might lead to some knowledge both of the *depth* from which they proceeded, and of the kind of *mineral strata* through which they burst. He was not disappointed in that hope: for he found among the *cinders* large fragments of *granite, sienite*, of several kinds of *quartzeous stone*, and of *hard limestone*; all of which belonging to the lowermost known *strata*, indicate clearly that the substance of which *lava* is formed lies *under* those *strata*.

Thus, when the real phænomena are attentively studied, nothing is left for the work of imagination. Is it permitted in presence of common sense to form *general systems*, from particular phænomena? Had the author considered this, he could not have supposed that a particular *saline substance* found on the slope of Vesuvius could lead to conclude that the *internal part* of the globe shows it to be *an extinct sun or comet*; while the *ejections* of that *volcano* demonstrate that the *lava* is formed under known *mineral strata* in which no sign of *igneous* operation is discovered.

Another great fact, with which the author appears to be totally unacquainted, though I have described it with all its circumstances in the abovementioned work, *Histoire de la Terre et de l'Homme*, is this: On the surface of our continents rise many *volcanic eminences*, showing indubitable characters of having been produced on the *bed of the sea* before it became our *continents*.

That great geological fact of our *continents* having been formerly the *bed of the sea*, so contrary to the author's system, is

demon-

demonstrated, as I have already had occasion to mention, by the immense quantity of *marine exuviæ* inclosed in the *secondary strata*. Now this character is found in the same countries as the *volcanic eminences* that are now the object, which I have particularly described, along the left bank of the Rhine, between Coblentz and Bonn, and in the country of Hessia. These *eminences* have all the characters of the actual *volcanoes*, and in particular of Vesuvius. In them and around them are seen not only *lavas* of different degrees of porosity and of mere *scoriæ*, which successively flowed over each other, but between them layers of *cinders* and an immense quantity of *pumice-stones*.

The *pumice-stones* are a known circumstance belonging to Vesuvius, but with a very remarkable character. It is not an ejection from the *crater*, or from any *known* part of that *volcano*; it rises from the *bottom of the sea*, in parts where the water is very hot. My brother had the opportunity of discovering that circumstance by a dog, who was fond of swimming by the side of his boat, and sometimes cried out as being scorched by the heat; and my brother actually found the water hot by plunging his hand into it. These are probably the places whence the *pumice-stones* come up from the bottom of the sea: they are found floating on the surface of the sea, as *pumice-stone* is specifically lighter than *salt water*.

Now this very important and known *volcanic product* is found around some *volcanic eminences* near the Rhine. I have seen very extensive beds of *pumice-stones* on the left bank of that river opposite to Coblentz; and in some places these beds of *pumice-stones* are intermixed with distinct beds of *cinders*, thrown up at some intervals from the *crater* of these *volcanoes*. But on this subject, as on the *aqueous origin* of our *mineral strata*, there are too many circumstances all to be mentioned here; I must therefore refer the author to my descriptions.

These are not the only remains of ancient *volcanoes* observed on our *continents*. In a new geological work which I have lately published, under the title of *Geological Travels through some Parts of France, Switzerland and Germany*, I have described in the northern parts of the latter country a multitude of *basaltic hills*, which evidently are *volcanic* from the nature of their substance; but they are particularly interesting, as they afford a new proof that some *lavas* come out from the *bottom of the sea*: these *lavas* from their nature, when meeting red-hot the water of the sea, were broken into the *prismatic* form of *basaltes*.

Of this effect my brother has seen an instance belonging to modern *volcanoes* on the coast of Sicily, near Catana. A *lava* having flowed from the side of Mount Ètna down to the sea, the part which remained on the *land* retained the character of all

lavas;

lavas; but that part which entered the sea is seen, at the time of low-water, to be divided into the *prismatic* form which characterizes *basaltes*.

I hope, Sirs, that Mr. SMITHSON will read with interest in your Journal the statement of various geological facts which he had not had the opportunity of observing himself. He will thus understand, surely, that *geology* is too extensive a science for a general system being formed with any certainty, without having observed the different characters of the surface of the earth, not in one country only, but in many; as in some countries the same phænomena are attended with others which may prevent mistakes. And with respect to information received from countries unknown to ourselves, he will also conceive that no reliance is to be placed on the observations of those whose accounts manifest that they have considered the phænomena with a mind prejudiced by some previous hypothesis to which they are bigoted.

I have the honour to be, sirs,

Your most obedient servant,

To Messrs. Nicholson and Tilloch. J. A. DE LUC.

XXVII. *On the Phænomena of Sleep. By a Correspondent.*

THE celebrated physiologist Dr. Whytt suggested that congestion of blood in the head was the immediate cause of sleep. Though this position was supported by a large induction of facts, it soon fell into disrepute, upon the discovery of congestion under circumstances no way analogous, as in lunacy, convulsions, insanity, and intoxication. Dr. Whytt's views however have lately been revived, and extended with some modifications to an ample explanation of all the phænomena. It is now ascertained that there is a congestion of blood in the head during sleep; but that congestion alone is insufficient, a retarded circulation being also necessary. For this modification we are indebted to Dr. Park, the author of "An Inquiry into the Laws of Animal Life." From this work, which is a general outline of physiological science, the grounds upon which the phænomena of sleep may be explained are chiefly extracted. This writer reasons entirely on an inductive or analytical process: that which I shall use will be synthetic, applying his principles to a very minute statement of facts, and occasionally advancing ideas of my own.

Two important facts are established in this work; one relating to the connexion between the function of every organ and its circulation, and the other to the changes incidental to the circulation itself. The former shows that the faculty of feeling and the power

power of motion are subject to change from every change in the vessels. Thus, when their action is diminished by cold, sensibility and mobility are both impaired, while both are increased by warmth. In like manner a part is rendered morbidly sensible by inflammation, but restored to its natural state again by abstraction of blood. The same observation holds equally true with the function of mind, which changes with every change of circulation in the brain. Thus, when the action of vessels is moderately increased by wine, we feel exhilarated, and our ideas flow more rapidly; if hurried by taking it to excess, the powers of association are confused, and by inordinate congestion they are altogether suspended; and a state of stupor comes on. On the other hand, diminished circulation has an opposite effect; the energies of mind are enfeebled by it; and if the abstraction of blood be excessive, fainting and a suspension of mental powers will ensue. The second fact illustrated in the work alluded to establishes the nature of the changes incidental to the circulation, and points out the various modes, whether natural or artificial, by which the particular state of the vessels inducing sleep may be brought on. It is shown that the vessels, in common with all moving organs, undergo periodical changes during action, which ultimately render them incapable of continuing it, and dispose them to relax. While congestion thus results from changes natural to all organs, this state of relaxation is also shown to admit of being accelerated or retarded by a variety of accidental and artificial means, and these it will be a principal object to investigate as we proceed.

Sleep, then, results from two combined causes: 1st. A congestion of blood in the brain; 2dly, A retarded circulation;—and it will be found that every thing inducing these conditions promotes sleep, while circumstances of a different tendency prevent its approach. The horizontal posture of the body facilitates sleep, because, in that state, the heart is relieved from the pressure of some pounds of blood, which, by the feeling of distension, excites the vessels to action. The limbs also being at rest, do not employ such an exertion of muscular power as assists to make circulation more rapid. From the diminished feeling of distension follows a relaxation of the vascular system, and moderate congestion of blood, with retarded circulation, and consequent sleep. If, however, the congestion be immoderate, it excites the vessels to inordinate action, and produces a contrary effect: hence it is difficult to repose without a pillow, or with the head so placed as to cause a rush of blood on the brain. Men however of a corpulent and plethoric habit find the horizontal position not so convenient as a reclining posture in a chair, with the head hanging down. In persons of this description there is already

already a congestion nearly sufficient for sleep, so that a furthee increase produced by the horizontal posture would excite inordinatr action. Nothing is required but a circulation somewhat slower than ordinary, which, in vessels habitually inactive, readily ensues from the unequal pressure on the seat, and the obstruction of the dress, rendered more tight round the neck and waist by the posture of the body.

Exercise brings on sleep by the relaxation of vessels, which naturally follows their continued exertion. Hence the sound rest of labouring men; while persons confined to the house sleep very lightly, their vessels not having been so far fatigued as to undergo a full relaxation. Children are found to sleep more frequently and more soundly than old people; the playfulness of the former soon inducing fatigue, more especially on account of their great mobility of fibre,—whereas the latter have few excitements to personal exertion. To this we may add, that the circulation in childhood is so full, that a slight retardation of the natural action creates the requisite conditions, while in old age it is difficult to cause much congestion without exciting a degree of action, which at that time of life is inordinate. Fatigue will induce sleep even under the most extraordinary circumstances, frequently during intense cold, though the person is conscious that he can never wake again. The cold in this case cooperates with lassitude, by impairing the sensibility of the vessels. The harassed soldier will repose in the trenches, and the malefactor before execution will yield himself up fatigued in mind and body by his violent emotions. To the same head may be referred the cruel artifice of some nurses to make children sleep. This plan consists in frightening them with the idea of some terrible object being in the room, so that they lie perfectly still, not daring to move a muscle. This want of motion retards circulation, while terror, moreover, constricts the vessels so forcibly, though nearly exhausted with the play of a whole day, that relaxation and congestion must soon succeed. If fatigue has been such as to leave the muscles in a state approaching to inflammation, the period of rest will be a while retarded by painful distension and inordinate action of vessels.

It is found that sleep is induced by fixing the mind permanently on one impression. This phænomenon may be referred to the principle, that attention to any one impression diminishes the feeling of others, and consequently the action which, when felt, they would produce. If therefore the mind be diverted from attending to the feeling of distension in the vessels, they will be less excited to action, will gradually relax, and ultimately undergo congestion. Hence we are lulled by the murmuring of waters, the humming of bees, the droning sounds of a heavy speaker, and the

the monotony of a book too dull to afford a single idea striking enough to divert the attention from its general uniformity. In the same manner, in long stories of one tenor, we at last only hear the voice of the narrator, and finally fall asleep. The exclusion of light, and the absence of promiscuous noises, by removing stimuli to the attention, have the same tendency. Regular and gentle motion, by fixing the mind on its own uniform impression, lulls us to rest, as we daily witness in the effects of rocking upon children. Thus also, if the head of a fowl be placed under its own wing, and it be moved gently in a vertical circle, a few turns will lay it completely to rest. Here the warmth of the wing relaxes the vessels of the head, in which congestion is promoted by contortion of the neck, while the uniformity of motion completes the effect. So also children are told to shut their eyes, and look steadily for some person that they will soon see; and it is found that, while their attention is so directed, sleep very soon overpowers them. Grief, when fully settled upon one object, has also a tendency to bring on heaviness, in which case congestion in the head is evinced by red and swollen eyes, with other symptoms of distended vessels.

It is further found that every thing which causes a gentle relaxation of vessels, will, by the concomitant congestion and retarded circulation, contribute to sleep. Hence nightcaps, comfortable beds, and the warmth of a fire after dinner, will all promote drowsiness. If however the warmth be so great as to cause immoderate congestion, with an accompanying inordinate action in the circulating system, sleep is prevented, as we find by the uneasy restlessness arising from nightcaps too thick and beds too downy. As internal impressions excite contraction, so those which are external cause relaxation; and upon the principles stated, the latter ought to create sleep. As pleasurable feelings operate in a similar manner, their effects ought to be the same. Hence we incline to sleep by the pleasing sensation of a full meal, which at the same time operates by its external impression on the vessels of the stomach, already predisposed to relax by the exertion of the day. If, however, the system be not predisposed to relax fully, a hearty meal rather exhilarates, by augmenting vascular motion; and if a heavy supper be taken, sleep is disturbed from irregular circulation, which results from its unequal pressure on the arterial system. From the relaxation following external impressions, children and the inferior animals are lulled to sleep by a gentle friction of the head. On the principle that the idea of an impression produces effects similar to the impression itself, some persons feel a propensity to sleep from thinking of it, or become drowsy in company from the very apprehension; and we find birds go to roost in a total eclipse of the sun. From habit also the system

relaxes

relaxes at stated intervals, so that a lady of fashion will be quite lively at a period when the regular citizen is seized with an irresistible drowsiness. Habit will, moreover, so far divert the attention from feelings generally causing action, that some persons can sleep while standing upright, or sitting on horseback.

Upon the principles laid down, it will not be difficult to explain the operation of some other means by which sleep may be promoted or prevented. If the skull be laid open, and the cerebrum gently pressed upon, the animal sleeps from the retarded circulation and congestion induced. Opium and extreme cold, by impairing the sensibility of the vessels, diminish their resistance to the contained fluids, thus retarding circulation, and removing any opposition to congestion. Wine taken to excess, after a certain period naturally disposes to sleep; for its primary effect being to increase circulation, and accumulate blood in the head,—as soon as relaxation succeeds to inordinate action of vessels, the brain will be under every circumstance requisite for repose. Strong tea, both by its sensible impression on the mouths of the vessels, and by the relaxing effect of warmth, promotes secretion, unloads the vessels, and so far removes congestion, and quickens circulation, as to promote wakefulness. Opium indeed causes relaxation of vessels, by impairing their sensibility; but instead of increasing it usually diminishes secretion, and does not therefore remove congestion, but increases it. Acids on the contrary, by promoting secretion, unload the vessels, and, like tea, remove the soporific effects of opium.

Such are the very numerous facts referable to Dr. Park's principles; and should any material points have been passed over, there can be little doubt that they also would reduce themselves to the same explication. He has extended his researches on this head to the phænomena of Dreaming, Incubus, and Somnambulism.

XXVIII. *Further Observations concerning the Production of the singular Substance called Iode, or Iodine; easy Methods of obtaining it; with Remarks on the comparative Nature of Kelp, as far as it regards the Preparation of Iode. By* FREDRICK ACCUM, *Operative Chemist, &c.*

Compton Street, Soho, Feb. 6, 1814.

SIRS,—THE obliging manner in which you have noticed in your Magazine for January last, the observations which I communicated to you, concerning the method of preparing iode, induces me to send a few additional remarks on the same subject, which I flatter myself may prove useful to some of your readers.

Upon

Upon a superficial examination of the varieties of kelp met with in the market, we soon become convinced that this substance differs prodigiously with regard to its external qualities as well as chemical composition. An extended series of experiments lately made upon different varieties of kelp, gives me reason to state, that some sorts contain, besides carbonate of soda and the usual salts peculiar to the sea-weeds from which kelp is obtained, a considerable portion of subcarbonate of potash. Others abound in sulphurets; again others, are almost wholly composed of common salt, with scarcely a notable portion of mineral alkali; whilst others again furnish from 2 to $3\frac{1}{2}$ per cent. of mineral alkali. In certain varieties of kelp we find a large quantity of sulphurets, and others again are almost free from sulphur.

This difference of composition is unquestionably owing to the mode in which kelp is prepared, as well as the articles of which it is manufactured. I am enabled to state from good authority, which I have received from different quarters, concerning the manufacture of this article, that in some places a considerable quantity of fern, [*Pteris aquilina* Linn.,] and woods of all kind, are burnt, together with the leafy *fuci*, for the production of kelp; and that in other districts, a mixture of common salt, sulphur, and the ashes of peat and brushwood, is added to facilitate the complete fusion of the ashes of the sea-weeds, to effect which, is said to require a considerable degree of practical knowledge of the kelp manufacture.

Without entering into a further detail concerning other abuses committed in the manufacture and sale of kelp, some of which are practised with a view of giving to British kelp the appearance of Sicilian or Teneriffe barilla, I shall confine myself, on the present occasion, merely to point out the characters by which the variety of kelp best suited for the preparation of iode, may be discriminated, from those sorts which contain almost none of this substance, or from which at least it cannot be obtained without much trouble and expense; and I shall likewise state the processes and practical proceedings which I have found to answer best to obtain this substance.

Kelp from which iode may readily be procured in the manner to be mentioned presently, is of a deep blueish-gray colour. It readily attracts moisture when exposed to a damp atmosphere, and then becomes intensely black. It is of a moderate hardness, and may easily be pulverized. Its recent fracture is minute cellular, porous, and earthy [not crystalline]. It emits a faint sulphureous odour when moistened with water or dilute acids. It possesses a saline alkaline taste. It affords from $2\frac{1}{2}$ to $3\frac{1}{2}$ per cent. of carbonate of soda, and when kept in a dry place it becomes covered

vered with a white efflorescence. By these characters kelp containing iode in considerable quantity may readily be recognised.

Those varieties of kelp which are of a greenish gray colour and crystalline fracture, which emit a strong sulphureous odour when moistened with water or dilute acids, which are of stony hardness, and do not readily attract water from a damp air, are almost unfit for the preparation of it; they yield but very little iode, and the means of extracting it from those varieties are tedious and difficult.

The best kelp is said to be manufactured in those places where the sea-weeds are cut down by means of a scythe from the rocks situated between the high- and low-water mark, as is the practice in the North of Scotland; and the worst kelp is said to be made in those places where it is customary to pull up the *algæ*, or leafy marine *fuci*, from the rocks by the roots, by means of rakes or grappling irons, as is practised by the peasantry in some kelp districts on the Irish and English coasts.

Those who are desirous of preparing iode [and who is there among the cultivators of chemistry that is not?] will do well to avoid useless trouble by first assaying the kelp they wish to employ for that purpose, before they proceed to operate upon it.

To accomplish this object, put a few ounces of it, previously pulverised, into a glass or earthenware funnel, and suffer eight or ten ounces of cold water to trickle or run over and through it, very slowly, so as to wash out the easily soluble matter which it contains.

To cause the water to pass readily through the mass, and to obtain the fluid clear at once, a few large pieces of kelp ought first to be introduced into the throat of the funnel; a stratum of smaller pieces should be put upon these, and the finer powdered kelp placed over all. When the water has run through the mass, pour the obtained solution back again upon the kelp in the funnel, and repeat this operation for several times. This being done, evaporate the lixivium to about two-thirds of its bulk; a considerable quantity of salt will become deposited during the evaporation; remove it from time to time, and lastly evaporate the remaining fluid to dryness. Transfer the mass into a crucible, and fuse it for a few minutes. From the appearance of the melted substances when cold, some conjecture may be formed concerning the nature of the kelp with regard to its contents of iode. If the mass exhibits a bright peach-blossom colour, we may predict that it abounds in iode; if it possesses a flesh or very pale rose colour, it contains a less quantity of this substance; and if it is colourless, porcellaneous, resembling white enamel, it then is almost destitute of it.

The presence of iode may likewise be rendered obvious, by adding

adding first sulphuric acid in excess to a lixivium of kelp freed by simple evaporation, of the greatest portion of salt which it contains; then evaporating this solution to dryness, and again heating it in a flask or retort, with an additional quantity of sulphuric acid. If it contains iode, the mass now will acquire a rose colour, more or less intense, according to the quantity of iode present; the vessel in which the assay is made will become filled with a beautiful violet-coloured vapour; and a dense yellow or orange-coloured fluid, which is sulphuret of iode, will condense and run down in striæ on the sides of the vessel. If the solution abounds in hydrosulphurets, the sulphur which becomes precipitated by the admixture of sulphuric acid should be removed before the solution is distilled with sulphuric acid.

OR,

The before-mentioned dry salt, containing the iode of soda, &c. may be heated for a few minutes with half a part of its weight of red oxide of lead, and then distilled with sulphuric acid. If it contains iode, a violet-coloured vapour will become disengaged.

Method of obtaining Iode.

To procure iode (or iodine) in the most expeditious and cheapest manner, from kelp, proceed as follows:

Fill a conical bag, made of coarse linen, or any porous material, with powdered kelp. Suspend the bag in some convenient manner, and suffer cold water to trickle through the kelp in as slow a way as possible. Four parts of water are thus sufficient to lixiviate one of kelp. Pour the lixivium which runs from the bag, back again upon the kelp, and repeat this operation for three or four times successively. The obtained fluid will be perfectly transparent, and almost colourless. Evaporate this solution in a Wedgwood-ware bason, and remove the salt which becomes deposited from time to time. The quantity of salt that falls down, will diminish as the lixivium becomes concentrated. When no more salt becomes separated during the ebullition of the fluid, or when the quantity deposited is but small, the solution may be evaporated to dryness. This being done, transfer into a crucible the obtained saline mass which contains the iodine of soda, &c. and expose it to a dull red heat. When the mass begins to melt, a lambent pale blue flame will make its appearance; and when this has ceased, pour out the melted matter upon a slab, pulverise it coarsely, and preserve it for use.

To obtain iode from this substance, mix it with one-fourth of its weight of red oxide of lead, and decompose it with sulphuric acid, taking care to add this fluid as long as any effervescence takes place, and proceed as stated No. 189, page 57, Phil. Magazine.

If

If the operation is to be performed on a tolerably large scale, that is to say, if a pound, or pounds, of the partly-desulphuretted salt are operated upon at once, the process is most commodiously performed by means of an alembic. The iode sublimes in the capital of the alembic in beautiful prismatic needles, possessing a metallic splendour; and the residuary saline mass may readily be removed by the assistance of warm water.

If two or three ounces of the salt are submitted to the operation, the following process is convenient:

Take a flask holding three times the bulk of the salt, adapt to the neck of it, by means of luting, a glass tube about 12 or 18 inches long, and from 3-8ths to half an inch in the bore. Pour into the flask first, two parts of sulphuric acid, and then, one part of the uncrystallizable salt containing the iode. A violent action ensues, and the flask becomes filled with a dense violet-coloured vapour. When the action has ceased, apply a gentle heat till no more violet-coloured fumes become disengaged. Then remove the tube, into which the iode will be found sublimed. Wash out the crystals with small portions of water, and dry the product without heat on unsized paper, placed on a chalk-stone. The iode thus obtained is not pure. It is usually soiled with some sulphuret of iode, and hence has a strong odour resembling chlorine. It stains every part of the skin, and other animal matter, with a bright orange colour; the stain lasts for some days, and only disappears by the natural change of the cuticle, so that in dead matter it is indelible. In this state it becomes converted by heat, not into a violet-coloured gas, but into a rose-coloured gazeous fluid.

To obtain the iode in a pure state, mingle two parts of it with one of chalk freed from water by a previous exposure to a moderate heat, or with quicklime; put the mixture into a glass tube, eight or ten inches long and three quarters of an inch in the bore, furnished at one extremity with a ball, and then resublime the iode into the colder extremity of the tube, by heating the ball of the tube over a spirit lamp.

Another Process.

Having obtained the uncrystallizable saline mass containing the iode in the manner above stated, redissolve it in three or four parts of water, and add to the solution, sulphuric acid in excess; then evaporate the mixture again to dryness. Mingle the dry mass with about one-third of its weight of finely powdered red oxide of lead or black oxide of manganese, and distill with sulphuric acid added in excess, in the manner before directed.

Iode is to be found in abundance in the waste or spent lee of those soap manufacturers who employ kelp in the prepara-

tion of soap. To obtain the iode from the waste lee, let it be boiled for a few minutes with quicklime; strain the fluid, and mingle it with sulphuric acid in excess. This being done, evaporate the fluid to a syrupy consistence, and then distill or heat it in a flask as directed above with red oxide of lead and sulphuric acid. The iode will thus be obtained in abundance; and this in fact forms the cheapest process of obtaining it. If the waste lee contains much animal matter, soap, &c. it then is essential that they should be first destroyed by exposure to a moderate red heat.

Before I conclude this paper, I beg leave to state, that I have reason to believe that iode does not exist in barilla; although I spoke otherwise in my former letter. The kelp upon which I operated, being delivered to me under the name of a sample of barilla, was the cause of the deception into which I was led on that occasion. Indeed, there exists much confusion in the market between the articles of kelp and barilla. The latter is frequently adulterated or mixed with kelp in so dexterous a manner, that even the most skilful dealers in these commodities are *often* deceived. Those who have an opportunity of readily rendering the lixivium of kelp caustic, in the usual manner by quicklime, will do well so to do. The obtained salt, containing the iode, when then distilled with sulphuric acid and red oxide of lead, does not effervesce, and the distillatory vessels employed for that purpose may be of a less capacity.

I am, with respect, yours,

FREDRICK ACCUM.

Messrs. Nicholson and Tilloch.

XXIX. *On the Camphoric Acid, considered as a peculiar Acid.* By *M.* BUCHOLZ*.

M. BUCHOLZ, after having referred to the different memoirs which have appeared on the subject of the camphoric acid since the discovery of Kosegarten to that of M. Bouillon-Lagrange, has thought it his duty to publish his own observations on the subject.

The method of proceeding which appeared to M. Bucholz to be the best adapted for avoiding loss, consisted in treating two ounces of camphor with a pound and a half of nitric acid of the specific gravity of 1·250, to which he adds half a pound of pure nitric acid of the specific gravity of 1·550, rectifying the whole three times successively by employing a middling degree of heat, and taking away about half the liquor every time, or the quantity which is necessary that the whole of the camphor might be con-

* *Annales de Chimie,* tome lxxxiv. p. 301.

verted

verted into acid. After the second rectification, but a very small quantity of oil of camphor will be obtained, which floats above the weak nitric acid.

The camphoric acid, at the end of the evaporation, is found above the fuming nitric acid, under the form of a white and butyraceous substance. It is separated by means of a glass funnel, redissolved in boiling water and crystallized again, in order to separate from it all the excess of nitric acid. This acid perfectly pure weighed five drachms, exclusive of that which still remained in the concentrated nitric acid, and which might amount to one drachm and a half. This process therefore seems to abridge the ordinary manipulation. It appears that the great art consists in not pushing the distillation with too much activity, particularly towards the end of the operation, because we might run the risk of decomposing part of the camphoric acid formed by the nitric acid in excess, which may always be observed by the brown colour which the residue then assumes.

The form of the crystallized camphoric acid is feathery like that of the muriate of ammonia, as M. Bouillon-Lagrange has observed: this crystalline form is very different from all those of the acids composed of several radicals, and particularly from that of the benzoic acid.

*Comparative Experiments between the Benzoic Acid and the Camphoric Acid**.

Camphoric Acid.

1. *Solubility of camphoric acid in cold water.* Ten grains of camphoric acid were agitated in 500 grains of distilled water, in a vessel which might contain two ounces of water, for an hour and a half, at a temperature of 15° Reaumur; the liquor was then filtered on a filter weighing 45 grains. The filter when dried again had retained five grains of acid. Thus 100 parts of cold water dissolve one part of camphoric acid. This experiment differs from that of Kosegarten, who says that it requires 200 parts of water, and of that of Doerfurt, who pretends that

Benzoic Acid.

1. *Solubility of this acid in cold water.* Having operated under this circumstance as in the foregoing experiment, there remained a residue weighing seven grains and a half; consequently there had been only two grains and a half of acid dissolved: hence it results that one part of benzoic acid requires 200 parts of water to dissolve it. The benzoic acid differs considerably, therefore, from the camphoric acid in this respect. The result of this experiment also differs much from that of Doerfurt and other chemists, who assert that 500 or at

* Care was taken to employ acids perfectly purified by crystallization.

that it requires 400: it approaches that of Bouillon-Lagrange; for he had announced that this acid is dissolved in 80 parts of water.

If the above chemist has determined the quantity of acid kept in solution in water, by estimating what might be retained by a warm solution of this acid, when its temperature has fallen back to a lower one, he would have been right; for we know that similar solutions of salts constantly retain more salt than the water could dissolve at a temperature equally low.

2. *Solubility in boiling water.* One ounce of water was heated in a large vase to the boiling point, then camphoric acid was added until no more solution took place, and the whole was speedily weighed. It required nearly eleven parts of boiling water to dissolve one of camphoric acid. This result forms a mean between that of M. Bouillon-Lagrange who says that it requires 10, and Kosegarten who asserts that it requires 12.

3. *Solubility in cold alcohol.* 100 grains of alcohol were shaken with camphoric acid until the latter refused to dissolve at a temperature of 15° Reaumur. 106 grains of acid were dissolved: the liquor was of the consistence of a clear syrup.

4. *Solu-*

at least 400 parts of water are requisite to dissolve one part of benzoic acid.

2. The benzoic acid when treated in the same manner afforded a result which differed very little from that of the other chemists; for I found that it required $24\frac{1}{2}$ parts of boiling water to one of benzoic acid. Lichtenstein and Doerfurt have announced that 24 are requisite, and Trommsdorf has said 30. The benzoic differs considerably, therefore, from the camphoric acid in this property.

3. The same quantity of alcohol when shaken three times longer with benzoic acid could only dissolve 56 grains of it. This alone is sufficient to demonstrate the difference which exists between these two acids, since it must be admitted that it is the presence of the essential oil of benzoin which opposes the solution of the acid, this essential oil being very soluble in alcohol.

4. 100

4. *Solubility in boiling alcohol.* The cold solution of camphoric acid was boiled, and 40 grains more were gradually added to it: eight grains of alcohol were volatilized. Thus 92 grains of alcohol kept in solution 146 grains of camphoric acid, and this solution was very fluid. But it may fairly be presumed that this acid will unite in any proportion with boiling alcohol, since it melts by itself alone with heat. This solution goes into a crystalline mass at a temperature of 2° below zero.

4. 100 parts scarcely dissolve so many of benzoic acid: in this respect this acid differs much from the camphoric acid, since alcohol can dissolve more than double its weight of the latter.

5. *Action of caloric in close vessels.* 20 grains of camphoric acid were heated in a small retort containing an ounce of water, until the whole was volatilized. Gradually a humid vapour was formed which was condensed in the receiver, and presented two drops of liquid resembling water, which had an empyreumatic taste and smell, and at the same time the flavour of acetous acid. The empyreumatic smell was accompanied with that of turnips. The sublimate which was formed in the neck of the retort had this smell also: in short, it was a very white saline matter, with some brownish spots: it was opake, and was not sensibly crystallized, resembling in nothing the crystalline sublimate which is furnished by the benzoic acid. The taste of this matter was acidulous and piquant, which was occasioned by the empyreumatic oil, which also diminished a little its solubility

5. 20 grains of benzoic acid were heated in the same manner.

The volatilization was much more prompt than with the camphoric acid. At the commencement there appeared a slight current of white vapours which were condensed in the neck of the retort, under the form of a whitish dust, weighing about half a grain; afterwards the acid was sublimed as usual in the form of beautiful white demi-transparent needles. There was but a very slight trace of empyreumatic oil, and charry matter, but not the smallest vestige of humidity; the interior of the retort and of the receiver had a slight smell of benzoin. All these observations are very different from those presented by the camphoric acid, when it is sublimed; and certainly they do not proceed from an excess of oil, as Doerfurt pretended.

bility in cold water. This solution reddened turnsole paper. These two last experiments do not agree with those of M. Bouillon-Lagrange, who asserts that the sublimate is not soluble in water, and that it does not redden turnsole paper. But he must have certainly been led into an error by a part of the sublimate surcharged with empyreumatic oil.

The solution of this sublimed camphoric acid had also an aromatic smell like oil of rosemary: upon the liquor cooling, the acid was precipitated in the form of small leaves. The neck of the retort towards the belly was coated with a brownish empyreumatic oil, opake and very thick, weighing from a grain to a grain and a half: the bottom and sides of the retort were covered with a slight coating of charry matter.

In spite of the difference between the camphoric and benzoic acids as above exhibited, I thought it right to subject them to some new experiments, with a view particularly to neutralize them by some bases, in order to observe the properties of their salts.

6. *Neutralization of the camphoric acid by lime.* 50 grains of camphoric acid were boiled in four ounces of water, and it was attempted to neutralize them by pure carbonate of lime: when 30 grains of this carbonate were added, the effervescence ceased; but the liquor, although reduced to three ounces, reddened turnsole still strongly; it even retained this property when I had added 85 grains of carbonate of lime: having diluted the liquor in eight ounces of water, it lost this

6. *Neutralization of the benzoic acid by lime.* 50 grains of benzoic acid were boiled with 30 grains of carbonate of lime, and six ounces of water, until the liquor no longer reddened turnsole: the salt was afterwards separated by treating the deposit eight times with eight ounces of water each time. The residue, when well washed and strongly dried, weighed ten grains, and was nothing but pure carbonate of lime: thus 20 grains were consumed. The liquor having been

this property: it appeared, therefore, that the camphorate of lime always exists with an excess of acid. The liquor was decanted, and the deposit boiled with eight ounces of new water; it was then filtered. The carbonate of lime strongly dried weighed 57 grains: thus 50 grains of camphoric acid require, in order to be saturated, 28 grains of carbonate of lime. The liquor was reduced to half an ounce by evaporation in a small capsule: but as it refused to crystallize, another drachm of liquor was evaporated: a tolerably strong pellicle was then exhibited: upon the cooling of the liquor, there was separated at the end of 48 hours a considerable quantity of crystallized salt, but the form of the crystals was not sufficiently decided. The concentrated solution of camphorate of lime has not a remarkably salt or bitter taste; it leaves a taste of lime, and is a little acrid, which distinguishes it considerably from calcareous benzoate.

It must also be remarked, that always when this acid is saturated, there is a slight and transient smell of camphoric acid, of which however the liquor gives no indication even when it is heated.

7. *Action of caloric on the camphorate of lime.* Five grains of camphorate of lime were introduced into a phial with a long neck and narrow aperture. There was a slight noise, the crystals were then dried

been reduced to two ounces gave upon cooling small needle-formed crystals, silky and brilliant, several of which issued from a common centre. At the end of two hours the whole of the liquor was filled with these small crystals, and it furnished them to the end by evaporation and cooling: the taste of these crystals was mild, and a little earthy.

It is also proper to remark here, that although pure and white acid was employed, there was a slight smell of benzoic developed, which speedily disappeared, and which was no longer remarked in the liquor.

7. *Action of caloric upon benzoate of lime.* Five grains of this salt, perfectly pure and white, treated in the same way, presented the following phænomena: it was perfectly liquefied; there was an extrication of

dried without melting: the vapour of water and a keen aromatic smell were developed, which resembled greatly oil of rosemary mixed with a little of empyreuma, occasioned by an empyreumatic oil: there was no sublimate of crystalline matter: at length we obtained a little thick empyreumatic oil: there remained a residue of carbonate of lime mixed with a little charcoal.

of aqueous vapours and a little very liquid oil, the empyreumatic smell of which was very slight, but it had a strong taste of balsam of Peru. From the beginning, benzoic acid was sublimed, but this soon ceased: the residue was the same with that of camphorate of lime.

The production of an empyreumatic oil analogous to the balsam of Peru, and obtained from a benzoate of lime perfectly pure and crystallized, is certainly worthy the attention of chemists.

8. *Solubility of camphorate of lime in cold water.* Twenty grains of this salt well dried were shaken for an hour and a half with 60 grains of water, at a temperature of 15° Reaumur; 13 grains were dissolved, for there remained seven grains of dry salt; thus five parts of cold water dissolve a little more than one part of salt, or 100 parts retain 21 and two-thirds solution.

8. *Solubility of benzoate of lime in cold water.* Fifty grains of this salt treated like the camphorate with 60 grains of water, were not dissolved, for there remained two grains in the filter: 100 parts of water, therefore, at a medium temperature cannot dissolve more than five parts of this salt.

The salts formed by these acids vary therefore considerably, and may still serve to elucidate the nature of these two acids: the case is the same with their combination with potash, as I shall now show.

9. *Properties of camphorate of potash.* This salt does not crystallize until its solution has been brought to the consistence of a liquid syrup, and it is then spontaneously evaporated: we then obtain small crystals: these crystals melt in their water of crystallization, the liquor becomes brownish, and requires a long time before the salt is hardened: it has then a pungent and caustic taste, while the crystals

9. *Properties of benzoate of potash.* The benzoate of potash with a slight excess of acid crystallizes easily in small laminæ, or thin needles, which are not very soluble in water, since this salt requires 10 parts of water at the temperature of 15° Reaumur. (The neutral combination of benzoate of potash is much more soluble, since this salt attracts humidity from the air.) The acidulous benzoate

crystals are but slightly salt, a little aromatic, and bitter. This combination also retains the same properties when there is an excess of acid.

zoate reddens turnsole paper, it has a less acrid taste than the camphorate, and is even mild. If we redden it, a part of the acid is volatilized without being decomposed, but the major part is decomposed: empyreumatic oil is formed, and there remains charcoal with the alkali in the state of carbonate of potash.

The want of time and of a greater quantity of camphoric acid prevented me from prosecuting the foregoing researches: I shall content myself therefore with a brief recapitulation of the properties of these two acids:—

1. The camphoric acid is constantly crystallized upon cooling in the form of quill feathers, as observed by M. Bouillon-Lagrange. The benzoic acid in the same circumstances crystallizes either in needles or in laminæ, or under a ribband form.

2. The camphoric acid has a decided acid taste, and leaves a bitter smack. The benzoic acid is mild, sweetish, scarcely acid, and a little pungent.

3. The camphoric acid, in order to be dissolved, requires 100 parts of water, at a temperature of 15° Reaumur, and 10 or 11 parts of boiling water. The benzoic acid requires 200 parts of cold water, and a little more than 24 of boiling water.

4. $\frac{6}{100}$ of camphoric acid dissolved in one part of alcohol at a middling temperature, and it should seem that it is dissolved in all proportions in boiling alcohol. The benzoic acid is dissolved in two parts of alcohol at a middling temperature, and it requires weight equal to its own of boiling alcohol.

5. The camphoric acid is volatilized and sublimed, but the products are very different. *a*) It is denser. *b*) There is a greater quantity decomposed: a particular oil is formed, an acid liquor, and more charcoal remains. The sublimate never takes a crystalline form. The benzoic acid is always sublimed in crystals: aqueous vapours are never produced, scarcely is there any empyreumatic oil formed, and there remains but very little charcoal.

6. The camphoric acid united to the bases produces salts very different from those formed by the benzoic acid, as may be seen from the above experiments.

XXX. *Pro-*

XXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 27. The Right Hon. President in the chair. A long paper by Dr. Brewster was read, giving a detailed account of his numerous experiments on light, in addition to those which have already been laid before the Society. Dr. B. has investigated with great care the laws which govern the reflexion of light, and ascertained that its polarization discovered by Malus is not a general but a particular law. The phænomenon of double reflexion has engaged his attention, but it is not possible to convey any adequate idea of his new observations and discoveries without drawings in this brief report.

Feb. 3. Sir Humphry Davy communicated to the Society a long account of his very accurate and ingenious experiments on the fluoric principle. Fluor is one of the many substances which present great difficulties to the inexperienced operator, and promise no brilliant results to reward his labour; consequently it remained almost unexamined till the important discoveries of this philosopher naturally led him to investigate its nature and properties. His former experiments developed many new and curious analogies of nature; his present confirm the observation that hydrogen produces as many acids as oxygen, and that it unites to the peculiar base, which he calls fluorine, to constitute fluoric acid. His first opinion, founded on analogy, that silica and boron were metals, he here abandons, and considers them also as peculiar undecomposed substances, which, in the present state of our knowledge, must be admitted as elements, the same as oxygen, hydrogen, chlorine and fluorine. The result of many complex and delicate experiments is, that fluo-borats can never be produced without the aid of hydrogen; and finally that, contrary to the supposition of Berzelius and others, who have argued the point with at least as much zeal for distinction as truth, chlorine contains no oxygen.

A short paper by Mr. A. Carlisle was read on Monstrosities, as an appendix to his remarks on Zerah Colbourn; the purport of which was, that both sacred and profane history record examples of hereditary monstrosity, particularly in supernumerary fingers and toes; and that these extra-appendages are generally attached to the outer side of the hands and feet, very rarely to the inner, and almost never to any other limb.

Feb. 10. Mr. Brodie (through the Society for promoting a Knowledge of Animal Chemistry) communicated an account of his observations and experiments on the influence of the nerves on glandular secretion, particularly that of the 8th pair. In consequence

quence of his having found much secreted fluid in the stomach of dogs poisoned with arsenic, he determined to ascertain, if possible, whether such secretion was an effect of the poison or of the nerves; and after dividing the nerve which communicates with that organ, he inserted some arsenic into the thigh of a dog, which shortly after died; but on examining the stomach, although the usual appearances of inflammation were present, no secreted matter was found. The experiments were repeated and varied several times with similar results; nevertheless, the author admits that all such experiments on living bodies are inconclusive; that although it is extremely probable the nerves contribute to effect secretion, yet it cannot be demonstrated indubitably by any direct experiments; in other words, that all the positive knowledge acquired by his experiments is but a small atonement for the torture inflicted on so many animals.

A Letter from Mr. Kœnig to the Right Hon. President was also read, describing the fossil human skeleton brought from Guadaloupe to this country by Admiral Sir A. Cochrane, and deposited in the British Museum. This singular fossil was found on the shores of Guadaloupe below high-water mark, among calcareous rocks formed of madrepores, &c., and not very remote from the volcano called the Souffriere; the block containing the human skeleton is eight feet long, two broad, and weighs about two tons; it is a very hard granular limestone resembling calcareous sand-stone, containing a few venus and other shells, some of which are unknown. The skeleton is tolerably perfect, with the exception of the skull and some vertebræ of the neck, which are wanting. Sir H. Davy found some phosphate of lime in the bones, proving the presence of animal matter. Mr. K. does not pretend to guess at the age of this fossil skeleton; but Sir Joseph Banks, whose experience and observation are more extensive, considers it of very modern formation. Other fossil bones have been found in the same vicinity, and calcareous masses or rocks continue forming there. This circumstance seems to sanction the judicious opinion of the learned President; and taking into consideration the contiguity of a volcano, the probability of the temperature of the water being considerably raised at some times, and the known fact that carbonate of lime dissolved in water is afterwards deposited in a comparatively short period in masses of very hard and solid stone—every person may be convinced of the rapidity of the formation and also of the hardness of such stone, by inspecting the inside of tea-kettles in which water vulgarly called hard is boiled.

Feb. 17. The doctrine of animal heat, or rather the comparative heat of arterial and venous blood, has occupied the attention of Mr. John Davy, who communicated the result of his labours

bours in this department of science to the Society. The experiments of Crawford being performed at a time when the process and means of analysis were much less perfect than at present, it is necessary they should be repeated before they can be received as correct results in the actual state of our knowledge. Mr. Davy operated on the blood of sheep and lambs; and it must be confessed that the detail of his experiments will be read with more pleasure, that no animal experienced any pain from his researches. He began by depriving arterial and venous blood of fibrine, ascertaining their specific gravity, the former being 1047 and the latter 1050, placing them in glasses of equal dimensions, filling a similar glass with water raised to the same temperature, and observing their relative rate of cooling. In different experiments he found arterial blood $93\frac{7}{10}$ and venous 92, a result altogether incompatible with the theory of Crawford, but reconcileable with that of Dr. Black or the opinion of Mr. Brodie. The posterior portion of the brain he found from 1 to 2 degrees higher than the anterior, and both were as much lower than the rectum. The heat of the body generally diminishes in proportion to the distance from the heart. (This fact is not very consistent with the notion of the nerves occasioning animal heat, as its focus is not very replete with nerves.) In general the temperature of arterial blood was from 1 to $1\frac{1}{2}$ degree higher than that of venous; only one degree was observed between the heat of the blood in the left and right ventricle of the heart. A newly born child raised the thermometer to 96; after three days it rose it to 99. Mr. Davy also made a variety of experiments on all parts of the body, with a view of ascertaining their relative heat; he avoided all theoretical speculations, but seemed somewhat inclined to the supposition of Dr. Black respecting the origin of animal heat.

Part of a paper, by Mr. Ivory, on Comets, was read, in which this acute mathematician expounded the judicious theory of Newton, described their parabolic motion, &c.; but much of his communication was of a nature not to be read.

XXXI. *Intelligence and Miscellaneous Articles.*

Dr. Clarke's and Mr. J. Clarke's Lectures on Midwifery and the Diseases of Women and Children will commence on Monday, March 21st, at the house of Mr. Clarke, No. 10, Upper John-street, Golden-square, where they are read every Morning from a Quarter past Ten to a Quarter past Eleven, for the convenience of Students attending the Hospitals.—For particulars apply to Dr. Clarke, New Burlington-street; or to Mr. Clarke, at the Lecture-room.

LIST

LIST OF PATENTS FOR NEW INVENTIONS.

To William Spratley, of the Strand, in the county of Middlesex, coal-merchant, for his improvement upon the axle-tree of wheels for carriages of different descriptions.—20th Dec. 1813. —2 months.

To John Sutherland, of Liverpool, in the county of Lancaster, copper-smith, for his improvement in the construction of copper and iron sugar pans and sugar boilers, and a new method of hanging the same; and also an improvement in the construction of the furnaces or fire-places in which such pans and boilers ought to be placed.—20th Dec.—2 months.

To Sir Thomas Cochrane, knt. commonly called Lord Cochrane, for his methods of regulating the atmospheric pressure in lamps, globes, and other transparent cases for supplying combustible matter to flames, and preserving uniform intensity of light.—24th Dec.—6 months.

To Ralph Sutton, of Birmingham, in the county of Warwick, brass-founder, for his effectual security to prevent the accidental discharge of fowling-pieces, which invention is unconnected with the lock, and applicable to all kind of fire-arms.—24th Dec.—6 months.

To James Cavanagh Murphy, of Edward Street, Cavendish Square, in the county of Middlesex, architect, for his invented Arabian method of preserving timber and various other substances from corruption or decay.—24th Dec.—12 months.

Meteorological Observations made at Clapton in Hackney, from January 20, to February 13, 1814.

Jan. 20.—Therm. 28°. Barom. 29.30—29.70. NE. gale, cloudy; wild geese in the form of a V flying in a small flock, very low. Sea-gull seen on the Thames.

Jan. 21.—Therm. 16°. 22. Barom. 29.84. N. Clear cold day, and few clouds.

Jan. 22.—Therm. 24°. 19°. Barom. 29.80.—29.94. N. Gentle breeze A.M. overcast and a little snow; gale and clear P.M.

Jan. 23.—At noon Therm. 30. Barom. 29.85. Showers of snow from the N. at different times; the snow-flakes of different sizes; at night almost sleet fell.

Jan. 24*.—Therm. 26. Barom. 29.88.—29.84. N. Snow showers from N. The rooks, *Corvi frugilegi*, came in numbers into the garden,

* I understand this great and long frost, like the fog which ushered it in, began first in the western parts of the island, and travelled eastward and northward. Many of the highways are blocked up with deep snow.

garden, and near to the house, making a dismal and pitiful deep cry: during the snow I observed a nnmber on a tree, all with their heads to the N. facing the wind.

Jan. 25.—Clear A.M.; cloudy P.M.; falling Barometer. Therm. 11 P.M. 27. N. SW.

Jan. 26.—Barom. 29.70.—29.39. Therm. 34°. 33°. S. The sky was cloudy, with small snow, and the temperature remarkably uniform throughout the day.

Jan. 27.—Barometer 29.35.—29.20. Therm. all day 33°. Cloudy and thaw. S. Calm.

Jan. 28.—Barom. 29.30.—29.28. Therm. 33°. 30° Small sleet from the S.

Jan. 29.—The Barometer fell much during last night; at eight this morning was 28.58., and in the afternoon down so low as 28.25.! Therm. about 33°. Clouded, with storms of wind at night. SW.

Jan. 30.—Barom. rising again 29.05.—29.30. Therm. 33°. Wind got to NW. Floods much out in the marshes of the river Lea, particularly towards night, when the waters covered the road, and were over the foot-bridge several inches*.

Jan. 31.—Barom. 29.38.—29.58. Therm. 27° 35°. S. N. SW. *Stratus*—snowing.

Feb. 1.—Barom. 29.88.—30.10. Therm. 30°. 29°. NW.—NE. Clear morning; unwholesome kind of day; lobated *cirro-stratus* and thaw again P.M. (The lobated *cirrostratus* is a cloud having the shallowness and light evanescent texture of one kind of *cirrostratus*, but with the rounded superficies of the *cirro-cumulus*: when it supervenes on a frosty and clear day, it is often a token of a state of air which affects the nervous system, and produces illness in many people: I have noticed this repeatedly.

Feb. 2.—Barom. 29.95.—30.10. NW. Clear and frosty.

Feb. 3.—Barom. 30.10.—30.05. Therm. 24°. White frost and *cirrostratus*.

Feb. 4.—Clear—Coming warmth, as indicated by the cirrocumulative tendency of the light clouds by night. Barom. 30.10.—30.22.

Feb. 5.—Rather warmer; clouded sky, and thaw.

* I noticed during the thawing of the snow to-day, that before the snow had so far melted as to leave the ground in general bare, a circular disk of earth was discerned round the roots of the stems of the trees and shrubs in the garden; a circumstance which seems to show that heat was transmitted from vegetable bodies. If vegetables as well as animals did not possess internal sources of heat, it seems that their juices must be frozen during hard frosts, and life often destroyed. The circumstance above alluded to corroborated this doctrine, and inclines one to think that the communication of the warmth is more considerable than is usually imagined.

Feb.

Feb. 6.—Calm cloudy morning, with mizzling rain: at night blew a gale from SW. and a clearer sky, but falling Barometer.

Feb. 7.—Gale from SW. Confused and rainy appearances of *cirrus* and other clouds. Therm. 50. Many sea gulls, *Lari cani*, in the marshes.

Feb. 9.—Wind W. Some cauliflower-like *cirrocumulus* and other confused clouds. Barom. getting up to 30.00. The raven croaking while sailing round and round aloft.

Feb. 10.—SW. breeze. Clouded, damp and warm.

Feb. 11.—Various confused features of the modifications. Warm and muggy day. Wind SW. and S. Sea gulls abound about the marshes of the river Lea.

Feb. 12.—Overcast and dark day. The conflagration of the Custom-house occasioned a curious phænomenon this morning. Looking out of the window, I observed the air replete with detachments of a black light substance, which on minute inspection appeared to be fragments of burnt paper, coming over from the south in a very gentle wind, from the fire at the Custom-house. These papers must have been carried to a great height in the air; for after the lower current of wind got to E., which it did for a short time, I observed numerous portions of the said papers coming also from that quarter. Whether they were carried up aloft merely by the current of heated air from the fire, or whether the explosion of the gunpowder which took place heaved them up on high, is uncertain; their long continued appearance, and the extent to which they were carried, (for they went some miles into Essex,) seems to indicate the former cause.

Feb. 13.—Clouded still day, wind southerly. The floods, which have been more or less out ever since the thaw, are daily abating, and the water-mills on the Lea work again as usual.

Clapton, Feb. 13, 1814. THOMAS FORSTER.

P.S.—In a former paper, and also in my Researches about Atmospheric Phænomena, p. 92, I noticed that the changes of wind appeared to take place first above, and to be continued downwards. Some experiments made with Thermometers suspended at different heights, have induced me to believe the same thing to happen with respect to the changes of temperature. It would be a good thing to try this by repeated observations on Mr. Six's or the self-registering Thermometer hung at different altitudes along high-erected poles. The instruments might be drawn up and down the poles by means of strings hung over pulleys at the top. The old May-poles, which still remain in many of our country villages, might be converted to this use. Paper kites, too, might be made large enough to carry up Thermometers at their tails.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For February 1814.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Jan.	27	35	38	36	29·20	0	Cloudy
	28	35	39	30	·18	10	Fair
	29	32	41	35	28·38	0	Rain
	30	32	38	32	29·15	12	Fair
	31	28	36	30	·40	10	Fair
Feb.	1	28	36	29	·96	13	Fair
	2	35	40	30	·90	4	Fair
	3	27	34	26	30·01	7	Fair
	4	21	30	25	·08	5	Fair
	5	27	33	37	29·95	0	Snow
	6	40	44	33	·81	0	Showery
	7	35	43	35	·70	0	Showery
	8	38	49	40	·40	0	Showery
	9	39	48	44	·97	8	Cloudy
	10	43	49	46	·99	7	Cloudy
	11	44	50	45	·98	8	Cloudy
	12	44	49	42	·96	14	Fair
	13	42	48	40	·96	12	Fair
	14	41	46	34	30·20	9	Cloudy
	15	33	42	32	·28	9	Fair
	16	31	40	28	·30	6	Fair
	17	28	35	25	.39	5	Cloudy
	18	21	37	37	·30	6	Fair
	19	38	69	28	·35	9	Cloudy
	20	26	30	25	·40	5	Fair
	21	22	37	27	·34	5	Fair
	22	26	38	28	·30	6	Fair
	23	26	34	24	·26	6	Fair
	24	21	33	24	·15	7	Fair
	25	20	36	25	·16	6	Fair

N.B. The Barometer's height is taken at one o'clock.

XXXII. *An Attempt to determine the definite and simple Proportions, in which the constituent Parts of unorganic Substances are united with each other.* By JACOB BERZELIUS, *Professor of Medicine and Pharmacy, and M.R.A. Stockholm.*

[Continued from p. 101.]

II. LAWS FOR THE FORMATION OF SUBSALTS.

EVERY chemist knows what is understood by neutral salts; but it is by no means easy to give a good definition of what is properly *neutral*. If we take that condition of alkaline salts, in which both the acid and the base are perfectly indifferent, so as to produce no reaction on vegetable colours, as an example of neutrality, it seems that we ought to consider those salts only as neutral, in which the same quantity of oxygen in the base combines with the acid, as in these alkaline salts, and which in double decompositions would set neither acid nor base at liberty. Davy, in his Essay on Electricity as a chemical agent, calls every combination *neutral*, in which the original electric reactions have ceased. This is in fact the only correct and scientific conception of a neutral combination; but it is only relative. For, according to this determination, the oxygen in the protoxide of lead, for example, must be neutralised; it no longer acts on the greater number of bodies as a [negatively] electrical substance, yet still retains the same relation to more combustible bodies, for instance, to potassium. Exactly the same is true of neutral salts. While potass and soda saturate a quantity of sulphuric acid, which contains three times as much oxygen as themselves, so completely, that the acid loses its powers, this is by no means the case with the protoxide of zinc, the oxide of iron, alumina, or zirconia. The attractions of these bases being extremely weak, every substance, which comes into contact with the salts, makes an effort to deprive them of a part of the acid: hence the action of the acid is still perceptible, and the salts appear not to be neutral, although they are the most neutral combinations of which the bases are susceptible. We are accustomed to call them *supersalts*, because the acid possesses the strongest powers, and consequently exhibits its effects most distinctly on the taste, and on vegetable colours. But when we consider the combinations of the weaker acids with the stronger bases, we find, that the base always exhibits its powers the most obviously. Thus, for example, the common deliquescent carbonated potass is sometimes called a subsalt; although the carbonic acid is united in it with the same quantity of oxygen, as in the carbonate of baryta or of lime, and these three salts are consequently in the same com-

parative state of saturation. And that both these earthy salts are neutral, appears from the sufficiency of the force of cohesion in both to prevent the predominant actions of their very powerful bases.

We may therefore, I believe, consider all those earthy and metallic salts as *neutral*, in which the acid is united with as much of oxygen in the base, as is found in another decidedly neutral combination of the same acid with an alkali or an alkaline earth. Thus, I consider as neutral all those sulphates, in which the base contains one-third as much oxygen as the acid; and all arseniates, carbonates, muriates, and phosphates, in which the base contains half as much oxygen as the acid. Salts in which there is more oxygen than this in the acid, I call *supersalts;* and *subsalts,* those in which there is less.

In the first series of my experiments on definite proportions, I have adduced two examples of subsulphates. I thought that I had found that in the *subsulphate of the oxide of iron* the acid was combined with four times as much of the base as in the neutral salt, an opinion which appeared to be confirmed by a superficial examination of the *subsulphate of the oxide of copper.* But as I obtained more correct ideas on this subject, it appeared that the rule for the relation of the oxygen of the acid to that of the base by no means agreed with this determination: for, if the result of the analysis had been correct, the oxygen of the acid would have been $\frac{3}{4}$ of that of the base, and neither an integral multiple nor a submultiple. I therefore repeated the analyses with greater accuracy.

1. *Subsulphate of the Oxide of Iron.*

I dissolved some red oxide of iron in concentrated sulphuric acid; I heated the mixture until the acid was completely saturated, then dissolved the salt in water, filtered the solution, decomposed it with caustic ammonia, but so as not to precipitate the whole of the oxide, and digested the fluid for 24 hours upon the precipitate; which was then washed on a filter, as long as the presence of any sulphuric acid was indicated by the test of a salt of baryta. The salt, when well dried, had completely the appearance of the common precipitated oxide. When deprived of its water over a spirit lamp, which expelled from it nothing but pure water, it left a red powder, exactly like the colcothar of vitriol. Ten grammes of this powder, strongly ignited, left behind 7·98 of oxide of iron, and disengaged sulphurous acid during the operation: the remaining oxide, dissolved in muriatic acid, and tested with a salt of baryta, afforded no perceptible trace of sulphuric acid.

In this subsalt, therefore, 20·2 parts of sulphuric acid were united

united with 79·8 of oxide of iron, containing 24·47 of oxygen, while the acid contained 12·12; or, without any material error, half as much as the oxide. We see therefore that in the subsulphate of the oxide of iron the acid saturates six times as much of the base as in the neutral sulphate; for, according to the analysis of the neutral sulphate, which I have already described, 100 parts of sulphuric acid saturate 65·5 of the oxide of iron, and $65{\cdot}5 \times 6 = 393$. But, according to the present analysis of the subsulphate, 100 parts of the acid are combined in it with 395 of the oxide; and the difference is so small that it can only be attributed to an error of observation.

I then collected a quantity of orange-yellow ochre, which had formed itself from a vitriolic solution in a vitriol work; I washed it carefully, and dried it in the sun. When the water was expelled over a spirit lamp, it had lost 21·7 per cent. By ignition it lost 15·9 per cent. more of sulphuric acid, and the remaining red unmagnetical oxide amounted to 62·4 per cent. According to this experiment, 100 parts of sulphuric acid combine, in the subsalt, with 392·52 of the oxide, and the yellow ochre is thus constituted:

Sulphuric acid	15·9	100
Oxide of iron	62·4	392·52
Water	21·7	

The quantity of water contains 19·15, the oxide of iron 19·13, and the sulphuric acid 9·54 parts of oxygen. Consequently the base and the water contain equal quantities of oxygen, and the acid exactly *half* as much oxygen: so that, notwithstanding the great difference in their appearance, the *yellow ochre* and the *brown red precipitate* are exactly the same combination. I am sorry that the former analysis was made at a time when I had not begun to collect any observations respecting the water of crystallization.

I was not a little anxious to know why this result differed so much from my former analysis of the same salt, and therefore prepared some more of the compound in the manner which I had then employed; dissolving some iron in diluted sulphuric acid, to which, for the sake of increasing the quantity of the oxide, I had added a little nitric acid. When nothing more was dissolved, I put, in order to expedite the separation of the subsalt, a piece of polished iron into the filtered solution, and exposed it, in an open vessel, to a temperature of 25° or 30° [77° to 86°] for several days. I thus obtained a considerable quantity of an ochre-coloured powder, possessing the characteristic property of the subsulphate of iron which I first analysed; it was very little soluble in muriatic acid, and when dried scarcely at all so, and was not altered by caustic potass. When I first dried this pow-

der, after washing it well, in the sunshine, and then heated it in a small glass retort, I obtained in the receiver some water strongly impregnated with ammonia, amounting to 18·5 per cent. By ignition 32 per cent. of sulphuric acid was expelled, and 49·5 per cent. of red unmagnetic oxide was left behind. It appears therefore that, in my earlier experiments, the nitric acid, which I had employed for oxidating the iron, and which I then thought it unnecessary to mention particularly, had produced a combination of a totally different nature from the pure subsalt; and I had, in all probability, examined a mixture of these substances. This *ammoniacal combination* is very remarkable; it appears to be a *triple subsalt*, analogous to the ammoniacal copper. When heat is applied, the sulphuric acid, which had been in combination with the ammonia, unites with the oxide of iron, and the ammonia is set at liberty. This substance, in its difficult solubility in acids, and in its incapability of being altered by caustic alkalis, seems to approach in some degree to the triple combinations of ammonia with muriatic acid and tin, described by Davy, and to the combination with muriatic acid and phosphoric oxide. As I had probably obtained only a mixture of this substance with the subsalt of the oxide, I thought it useless to attempt a more correct determination of its component parts. But I shall endeavour on a future occasion to obtain the combination in a state of purity, and to examine the proportions of its constitution.

It is demonstrated by the analysis of the pure subsalt of the oxide of iron, that the relation between the iron and the sulphur, which I had inferred from my first analysis, is incorrect. We shall see in the following analyses, that in the subsulphates, the oxygen of the acid is either *equal* to that of the base, or an integral *submultiple* of it. And hence it will follow, that, in all the subsulphates, the sulphur is in such a proportion to the metal, that its quantity is an integral submultiple of the quantity in the sulphuret at a minimum, or of the quantity in the neutral sulphate of the protoxide of iron. In the salt of the oxide of iron here described, the proportion of the sulphur to the iron is exactly *one-fourth* of the quantity in the sulphuret at a minimum, and in the sulphate of the protoxide.

I must here call the attention of the reader to a circumstance, which is of the highest importance for the completion of the doctrine of definite proportions in mixtures, and without attending to which, we can scarcely hope ever to see the doctrine of the combination of organic bodies sufficiently illustrated: that is, to the existence of the *absolute minimum* of the combination of one substance with another, of which all other combinations must be multiples. Since only very few degrees of combination can exist between two bodies separately, that is, without the intervention

vention of one or more other bodies; and since these perhaps never, or at least very rarely, exhibit combinations at a minimum, we are obliged to seek for these minima in more complicated modes of combination. It will be difficult to discover the true minimum; but every good experiment, made with this view, will be productive of interesting consequences. The subsalt of iron, just analysed, may serve as an example in illustration of this remark. If we should hereafter discover no combination of the sulphuric acid with a greater quantity of iron in the salts of the protoxide or oxide, and should the proportion of the sulphur to the iron, found in this experiment, be the greatest common divisor of all the numbers expressing the proportions which are found in the mixtures of sulphur with iron, we might be allowed to hope that we had found the minimum of sulphur with which iron could combine; 100 parts of iron here being united with 14·66 of sulphur. In the magnetical pyrites, called the sulphuret at a minimum, because it is the lowest stage of combination which can be separately exhibited; or in the neutral sulphate of the protoxide of iron, 100 parts of iron are combined with $14{\cdot}66 \times 4 = 58{\cdot}64$, in the sulphate of the oxide with $14{\cdot}66 \times 6 = 87{\cdot}96$, and in the common pyrites with $14{\cdot}66 \times 8 = 117{\cdot}28$ parts of sulphur. Consequently these are multiples of the lowest proportion by 4, 6, and 8, and we find that these multiples agree with the results of the experiments as far as the thousandths of the whole. It may be supposed that the multiple by 2, which is here wanting, still exists, although perhaps in a combination which is yet unknown; for instance, in a subsalt of an oxide, in which the sulphuric acid and the oxide contain equal portions of oxygen. If now 14·66 parts of sulphur were the smallest quantity with which 100 parts of iron could combine, it would follow, that no subsalt of the protoxide of iron could exist. But if, on the contrary, such a subsalt should be discovered, 14·66 of sulphur for 100 of iron would not be the minimum, and it could not be greater than 4·9, which would be the greatest common divisor of all the combinations of sulphur with 100 of iron. Nearly in the same manner I have attempted to find the minimum of oxygen in the combinations of carbon; but in order to discover which of the various numbers that might represent the quantity of oxygen, is the true minimum, a great number of experiments would be necessary, which would require the labour of several years before they could afford a tolerably certain result.

2. *Subsulphate of the Oxide of Copper.*

I precipitated some sulphate of the oxide of copper with caustic ammonia, taking care not to throw down the whole of the oxide, and heated 10 grammes of the precipitate, well washed and dried,

dried, in a glass retort, over a spirit lamp, as long as any aqueous vapours were expelled. The salt, thus dried, had lost 14·5 per cent. of its weight. When I dissolved it in nitric acid, and added a salt of baryta to the solution, I obtained 8·55 gr. of ignited sulphate of baryta, corresponding to 21·28 per cent. of sulphuric acid. Consequently this salt consists of

Sulphuric acid	21·28	100
Oxide of copper	64·22	301·8
Water	14·50	

The sulphuric acid contains 12·74, the oxide of copper 12·66, and the water 12·87 of oxygen; so that each component part contains an *equal* quantity. Consequently in this salt 100 parts of sulphuric acid saturate *three* times as much oxide of copper as in the neutral sulphate.

3. *Subsulphate of the Oxide of Bismuth.*

Mr. Lagerhjelm found, in his experiments on bismuth, that, in the sulphate of the protoxide, the acid contains three times as much oxygen as the base. I therefore decomposed a quantity of the neutral salt by adding water to it, and washed the subsalt, which remained undissolved, by the addition of fresh portions of water. The salt, being long and thoroughly dried on a sand-bath, was then ignited in a crucible of platina, as long as a trace of sulphurous acid was disengaged. It had lost 14·5 per cent. of its weight, and consequently consists of

Sulphuric acid ...	14·5	100
Oxide of bismuth	85·5	590

But this quantity of sulphuric acid contains 8·685, and the oxide of bismuth 8·66 of oxygen: consequently the acid saturates *three* times as much of the base, as in the neutral salt.

4. *Subnitrates and Subnitrites.*

I have already treated, in the Second Continuation of my Essay, of the subnitrate, subnitrite, subsubnitrate, and subsubnitrite of the protoxide of lead, and of the subnitrate of the oxide of copper; and I have shown how far they serve to confirm my opinion of the composition of nitrogen, as well as the laws which I have here laid down for the formation of the subsalts.

5. *Carbonate of the Oxide of Copper.*

Ten grammes of carbonate of the oxide of copper, precipitated at the boiling temperature, and dried in the sunshine, being ignited in a small glass retort, afforded in the first experiment 7·16, and in the second 7·17 gr. of black oxide of copper. A considerable quantity of water was collected in the receiver. Consequently this salt cannot contain so much carbonic acid, as

to

to constitute with it a neutral compound; and the acid and the oxide must therefore contain *equal* quantities of oxygen; and 71·7 parts of the oxide must take up 19·73 of carbonic acid: the remaining 8·67 must be water, containing 7·5 of oxygen. This quantity of the oxide contains 14·34 of oxygen; so that the oxygen of the water is half as much as that of the base. The slight difference in the results must depend on a little accidental moisture left in the oxide, from the imperfection of the process of drying.

Carbonate of copper, precipitated from a cold solution, affords a very bulky powder, of a blueish-green colour: but when a boiling heat is employed, we obtain a heavy, fine-grained, yellowish-green precipitate. I at first considered these two precipitates as different carbonates, and attempted to collect the former, and to wash it with cold water; but it was converted by this process, in great measure, into the heavier yellow-green substance, and I could not obtain it in a pure state. I had accidentally placed on the sand-bath some carbonate of the oxide of copper, precipitated the day before, and still remaining in the fluid: when the carbonic acid had been expelled from the fluid, I observed that the carbonate of the oxide of copper, next to the bottom, collected into masses, and became of a yellow-green, without the least appearance of effervescence; and this effect extended by degrees upwards as the fluid became warm. The alteration in the form appears therefore to depend not on any alteration in the quantity of carbonic acid, but merely in that of water; exactly as the carbonate of the protoxide of zinc, in a temperature below the boiling point of water, sets at liberty the water combined with it, and unites into heavier grains, and as the blue hydrate of copper, when the fluid is heated, separates from the water, and is deposited in the form of a black oxide. Other subsalts of copper also, which, when they are precipitated cold, are light and bulky, become heavier by the effect of heat, and assume a yellower colour.

6. *Submuriates.*

We have found by some of the former analyses, that the muriatic acid, in the submuriates of the oxide of copper and of the protoxide of lead, is combined with four times as much of the base as in the neutral salt. Since the muriatic acid must contain twice as much oxygen as the base by which it is neutralised, the oxygen of the acid in these subsalts amounts only to one *half* of that of the basis.

7. *Conclusions.*

From these experiments I think myself authorised in drawing the following inferences respecting subsalts.

 a.) The

a.) The oxygen of the acid in a subsalt may be either a submultiple, or a multiple, by a whole number, of that of the base.

b.) The quantity of a base, which is combined with a given quantity of an acid in a subsalt, can be a multiple of the quantity in the neutral salt by such a number only, as is capable of expressing the proportion of the quantity of the oxygen of the acid to that of the base. For instance, in the subsulphates, the base can only be a multiple by 3, 6, 9, of the quantity in the neutral salt, and not by 2, 4, 5, 7, 8. On the other hand, acids which contain, in their neutral salts, 2, 4, 6, or 8 times as much oxygen as the bases, may take up in their subsalts 2, 4, 6, or 8 times as much of the base as in the neutral salts; but not 3, 5, 7, or 9 times.

It still remains to be inquired, whether there exists a subsalt, in which the base is less than double that of the neutral salt; for instance, whether any subsulphate exists, in which the acid contains twice as much oxygen as the base, or a subnitrate, in which the acid contains exactly four times as much oxygen as the base.

III. Laws for the Formation of double Salts.

The combinations which, in the first Antiphlogistic Nomenclature, were called *triple salts*, have been latterly more properly denominated *double salts*; being always composed of two neutral combinations; and they may commonly be obtained by mixing the solutions, and crystallizing them together.

These double salts may be of different kinds: containing either one acid and two bases; or two acids, or substances representing acids, and a single base. Of the former kind we are acquainted with several salts, of the latter with only a few, and with none very accurately.

1. *Double Salts of a single Acid.*

The principle of the formation of these double salts is not difficult to be found, since we know that most of the supersalts, if they are saturated with a second base, afford such salts, and it has been shown that the supersalts contain twice as much of the acid as the neutral salts. Consequently the new base must contain exactly the same quantity of oxygen as the base of the supersalt. For example, the soda, the protoxide of iron, or the protoxide of antimony, with which the supertartrate of potass is neutralised in pharmaceutical preparations, must contain as much oxygen as the potass, because it neutralises an equal quantity of the acid. We shall find from the analysis of alum, that the existence of the double salt, in these cases, depends not so much on the acid, as on the affinity of the bases; for alum cannot be obtained by sa-

turating

turating the superfluous acid of the supersulphate of potass with alumina, unless the supersulphate be in great excess. Besides, certain bases afford double salts with almost all acids, although most of the acids are incapable of forming a supersalt with either of these bases. Thus ammonia, for example, affords double salts with magnesia, with the protoxide of manganese, with the oxide of copper, and with the protoxide of zinc; and we have great reason to think that these bases always observe the same relation to each other, containing *equal* quantities of oxygen. I shall adduce some few examples of double salts of the *first* kind, which may be sufficient to illustrate the law of their formation.

Sulphate of Ammonia and Magnesia.

Ten grammes of this salt, finely powdered, and dried in the sun, were heated in a platina crucible, and then ignited. They afforded exactly one-third of their weight of sulphate of magnesia, whence the magnesia is found to amount to 11·11 per cent. and its oxygen to 4·43 per cent. of the whole weight. I now mixed, with these 3·334 gr. of sulphate of magnesia, a quantity of sulphate of ammonia, in which the oxygen of the ammonia amounted to ·443 gr. that is, 4·181 gr.: the two salts were dissolved in boiling water, and dried on a glass dish in the sunshine: their joint weight was found to be 10·006 gr. Consequently the sulphate of magnesia had taken up 2·49 gr. of water, containing 2·2 gr. of oxygen, that is, five times as much as either of the bases contained. But since the sulphate of ammonia contains water of crystallization of which the oxygen is equal to twice that of the base, the whole water of the salt contains seven times as much oxygen as that of each of the bases: and the respective portions of oxygen in the different substances entering into the combination are as 1, 1, 6, and 7.

Sulphate of Ammonia and the Oxide of Copper.

Ten grammes of this salt, finely powdered, and dried in the sun, were mixed with lime in a small retort, and the ammonia was expelled in the same manner as in the analysis of the sulphate of ammonia. The apparatus had lost ·827 of its weight. Ten more grammes, dissolved in water, were mixed with about as much of the carbonate of potass, as was required for the saturation of the sulphuric acid, and then evaporated to dryness. When again dissolved in water, they left behind carbonate of the oxide of copper. The fluid, which had a slight excess of alkali, exhibited, by the test of sulphuretted hydrogen, a slight trace of copper. The oxide of copper obtained, weighed, after ignition, two grammes. These contain ·3932 gr. of oxygen; and the ·827 gr. of caustic ammonia ·3897 gr.; so that the two bases contain

equal

equal quantities of oxygen. If now we compute the quantity of sulphuric acid necessary for their neutralisation, there will remain a quantity of water of crystallization containing seven times as much oxygen as either base. Consequently in this double salt, each of the two simple salts retains its appropriate water of crystallization, as appears from the respective analyses. The quantities of oxygen of the several component parts are here related, as in the former case, in the proportions of 1, 1, 6, and 7.

Alum.

Alum is generally considered as a double supersalt; but my analysis appears to show that it is in fact a neutral double salt.

Twenty grammes of pure alum were heated in a platina crucible, over a spirit lamp, till they lost no more of their weight. The swollen mass was compressed into the crucible, and at last covered, in order that the heat might pervade every part of it equally. It had now lost 9 gr. and I could find no trace of the escape of an acid, which must have been discoverable by the smell. Consequently alum contains 45 per cent. of water of crystallization. The dry salt was again dissolved in water, the assistance of heat being required for the solution; muriate of baryta was added, and the precipitate, when ignited, amounted to 19·973 gr. or very nearly to the original weight of the alum: so that, in a crystallized state, this substance contains 34·255 per cent. of sulphuric acid [; or, according to the corrected analysis of the sulphate of baryta, which appears to contain from 34·314 to 34·48 per cent. of sulphuric acid, at least 34·27 per cent. *Gilbert.*].

Ten grammes of alum were dissolved in water, and digested with an excess of ammonia: they afforded alumina, which, when well washed and burnt, amounted to 10·67 per cent. As this did not agree with Mr. Thenard's result, I repeated the experiment with 50 gr. of alum. After filtration, the fluid mixed with the excess of ammonia, and the water, with which the earth was washed, were evaporated to dryness, and redissolved in water, when they afforded a small quantity more of alumina: the whole, being collected and ignited, weighed 5·43 gr. and lost nothing more of its weight by repeated ignition. Consequently alum contains only from 10·67 to 10·86 per cent. of alumina.

Ten grammes of alum, dissolved in water, and digested in a small glass flask with carbonate of strontia, as long as any effervescence existed, and then with new portions of the same carbonate added in excess, were deprived in this manner of the whole of the sulphate of alumina. The precipitate was not swollen, as might have been expected from the alumina, but heavy and easily washed. The fluid, when filtered, was not alkaline,

nor

nor did it become turbid upon the addition of ammonia. When evaporated, together with the water used for washing the powder, in a platina crucible, and ignited, it afforded 1·815 gr. of sulphate of potass, answering to ·981 gr. of potass.

Since we have seen that alumina contains about 46·7 per cent. of oxygen, it is impossible that the bases can here contain *equal* parts of oxygen. Alum, according to these experiments, is thus constituted:

Sulphuric acid ..	34·23	Or,	
Alumina	10·86	Sulphate of alumina	36·85
Potass	9·81	Sulphate of potass ..	18·15
Water	45·00	Water	45·00

Now, 9·81 parts of potass neutralise 8·37 of sulphuric acid, and 25·86 parts of sulphuric acid remain for the alumina: consequently the alumina saturates in the alum three times as much acid as the potass, for $8·37 \times 3 = 25·11$; so that the alumina must contain three times as much oxygen as the potass. But 9·81 parts of potass contain 1·674 of oxygen, and 10·86 of alumina 5·077; and $1·674 \times 3 = 5·022$. The 45 parts of water contain 39·71 of oxygen, and $5·022 \times 8 = 40·17$. It is true that this analysis is not correct to the last places of decimals: but it is at least sufficient to prove, that in alum the alumina contains three times as much oxygen as the potass; and in this instance we have a double salt, in which the oxygen of one of the bases is an integral multiple of that of the other.

Since the proportion of the alumina to the sulphuric acid, in this analysis, agrees as nearly as possible with that which was found in the neutral sulphate, it is impossible that alum should be a supersalt; but it is indebted for its acid properties to the weak attraction of the alumina to the sulphuric acid which belongs to it; the sulphate of alumina preponderating so much above that of potass, that it communicates to the compound almost all its outward characters.

Alum and the other double salts afford us interesting examples of the combinations of more than two oxygenized bodies. The potass here contains the smallest quantity of oxygen, which must therefore be the common divisor for the quantities contained in the other component parts. If we call this quantity 1, the alumina will contain 8, the sulphuric acid 12, and the water 24.

Finally, I must observe, that my analysis of alum differs in some measure from that of Thenard and Roard. These chemists found in alum $12\frac{1}{2}$ per cent. of alumina, and only 16 per cent. of sulphate of potass. [Vauquelin found the component parts always 10·5 of alumina, 10·4 of potass, 30·52 of sulphuric acid, and 48·58 of water: the acid and the water taken together agree nearly with Berzelius's result: the alumina is a little less, perhaps

haps from the same causes of error which affected Berzelius's first experiment. *Gilbert.*] MM. Thenard and Roard employed 489 grammes of alum, from which they obtained 61 or 62 of alumina, a quantity which, when moist, would occupy the bulk of ten pounds of water, and which must have been received either on a very large filter or on several small ones: in both cases, the washing it, and its separation from the paper, must have been subjected to great difficulties. It is also probable, that the separation of the sulphate of potass from the fluid obtained, by the addition of lime, could give no very correct result. Probably therefore the difference of our determinations is rather to be attributed to the more or less appropriate methods employed than to the experimenters. Besides, it is certain that an analysis on too large a scale can never afford a very correct result; nor do these gentlemen appear to have been in pursuit of very minute accuracy in their experiments.

A double Subsalt.

There exist also some double subsalts; but I have hitherto examined only one, that is, the combination known in pharmacy under the name of *cuprum ammoniatum*. In order to prepare this salt, I dissolved some sulphate of the oxide of copper in caustic ammonia, precipitated the double salt with alcohol, washed it again with alcohol, and dried it in the air. It is very difficult to observe with accuracy the moment of the attainment of perfect dryness; for the salt is decomposed on the surface, before the alcohol has been expelled from its internal parts; it then becomes by degrees of a sky-blue colour, and at the edge green. Hence it is impossible to obtain a very correct analysis of this salt; but it will not be difficult, with the assistance of the laws of combination which have been here developed, to discover its true composition, since the result of the analysis cannot deviate far from the truth.

I drove off the superfluous ammonia from a part of this salt, on a sand-bath, until it became quite gray; it had lost 20·33 per cent. in weight. When I repeated the experiment in a small retort, I found that a little water escaped at the same time. The gray powder, when water was poured on it, became first of a light blue, and then, as I gently heated it, of a dark-brown colour. This unquestionably depended on the decomposition of the salt, and the formation of a portion of neutral sulphate of ammonia and copper, while some hydrate of copper was first produced, and then decomposed by the application of heat. The solution had a faint blue colour, and left 40 per cent. of sulphate of ammonia, mixed with a small quantity of the double salt. The part undissolved, being a mixture of black and green oxide, amounted

amounted to 48·7 per cent.: so that the products obtained weighed 9 per cent. more than the salt employed. This partly depends on the quantity of water of crystallization in the sulphate of ammonia, and partly on that of the new double salt.

In order to determine more correctly the composition of this subsalt, I dissolved 5 grammes of it in water, saturated the solution with muriatic acid, and precipitated the sulphuric acid with muriate of baryta. The precipitate, when washed and ignited, weighed in one experiment 4·685, and in another 4·7 gr., answering to 32·25 per cent. of sulphuric acid.

The superfluous baryta was thrown down with sulphate of soda, and then the filtered solution mixed with carbonate of potass, and evaporated to dryness. The mass, when redissolved, had an excess of potass, and afforded a greenish solution; the potass was nearly saturated with muriatic acid, and the carbonate of copper, received on a filter, washed, dried, and ignited. The liquid, still a little alkaline, exhibited by means of sulphuretted hydrogen a small residuum of copper, which being separately ignited, and weighed with the rest, made together 1·7 gr., or 34 per cent. of oxide of copper. We therefore find in this salt the same proportion between the acid and the oxide of copper, as in the neutral sulphate, and its properties as a subsalt are wholly dependent on the ammonia. But does the salt contain a quantity of ammonia capable alone of forming a neutral salt with the same quantity of sulphuric acid? I was at first persuaded that it did.

I mixed, in order to examine this, 5 grammes of the same salt in a small glass retort, with finely levigated lime, and decomposed it exactly in the same manner as I have related respecting the sulphate of ammonia. The little apparatus had lost 1·32 grammes, consequently the salt must have afforded 26·4 per cent. of ammonia. The 7·35 per cent. wanting must have been water, so that the *cuprum ammoniatum* is thus constituted:

Sulphuric acid	32·25
Oxide of copper ..	34·00
Ammonia	26·40
Water	7·35

This quantity of the oxide of copper contains 6·68, and the water 6·5 parts of oxygen, so that the oxide and the water are in the same proportion in this salt as in the subsulphate of the oxide of copper. The ammonia contains 12·424 parts of oxygen, or about twice as much as the other component parts; for we have seen that it is impossible to exhibit this salt in a state of dryness, without depriving it of a little of its alkali; so that the quantity must appear too small in this analysis.

It is evident that the two bases taken together here contain an

an *equal* quantity of oxygen with the sulphuric acid, that is, the oxide of copper one-third and the ammonia two-thirds as much. Consequently this salt is so constituted, considering both its bases, as to agree with the rule for a simple subsulphate. The quantities of oxygen are expressed by 1 for the oxide, 1 for the water, 2 for the ammonia, and 3 for the sulphuric acid.

It is not so easy to determine the nature of the alteration which the *cuprum ammoniatum* undergoes by exposure to the air. But it appears, when long kept in vessels imperfectly closed, so as to fall into a sky-blue powder, to lose half of its ammonia, so that the sulphuric acid then stands to each base in the same relation as in the neutral salts. When it is changed to a green powder, a still greater quantity of ammonia is lost, and the residue is a mixture of more or less dry neutral sulphate of ammonia with subsulphate of oxide of copper, accordingly as the alteration has been made by the effect of heat, or by exposure to the air. The green powder formed by heat is capable of supporting a temperature somewhat higher, without being decomposed, but afterwards emits sulphurous acid, sulphite of ammonia, water, and nitrogen, and leaves in the retort a fused dark-brown mass, which, when water is poured on it, affords neutral sulphate of the oxide of copper and red protoxide of copper.

It is probable that all other acids are capable of forming similar double salts with these two bases; but they cannot be so easily exhibited, because they are more soluble in alcohol.

If it were permitted to ground a general rule on a single example, I should conclude that, when an acid is supersaturated with two bases, both of them, taken together, contain the same quantity of oxygen as a single base must do, in order to form a subsalt with the acid; and that the oxygen of the one base must be a multiple of that of the other by 1, 2, 3...

2. *Double Salts with two Acids, or Substances representing Acids, and one Base.*

These salts have been little examined, and their number seems to be small. The only examples, in any degree well established, are afforded by the combinations of a base with sulphur and sulphuretted hydrogen, and with sulphur and sulphuric acid. It is probable, that in these cases the two [negative] substances divide the [positive] one between them, and take up either equal portions of it, or such portions as are in the proportion of 1 to 2, 3, or 4.

I have mentioned, in the First Continuation of my Essay, on occasion of the investigation of the relation of the oxygen of the acids to that of the bases of salts, a double salt consisting of the nitric and arsenic acids with protoxide of lead. When I endeavoured

voured to analyse this salt, I found its component parts indefinitely varying, accordingly as the solution from which it was crystallized was more or less concentrated. Since also it is decomposed by solution in water, it does not appear that it can be considered as a double salt: and it is perhaps only an intimate mixture of crystallized particles of the nitrate with those of the arseniate of the protoxide of lead.

Another similar instance has also occurred to me. I had mixed together a solution of muriate of ammonia and of muriate of the oxide of iron, and, having concentrated them, left them to crystallize. The salt which I obtained had formed cubes of a fine ruby colour; and upon analysing them, I found only $1\frac{1}{4}$ per cent. of the oxide of iron. When dissolved in water, they lost their colour, and I obtained from the solution first an almost colourless, and then a light reddish-yellow salt, which in some parts was free from any tinge of colour, and in others was unequally coloured, and only of an orange red. Consequently this triple combination is to be considered rather as a mixture, than as a true double salt.

[To be continued.]

XXXIII. *Observations on Electrical and Chemical Terms.* By *Mr.* J. Murray.

Colchester, March 4, 1814.

Sirs,—I thank you for your prompt insertion of my paper in No. 189 of the Philosophical Magazine and Journal. Be pleased to supply the word *except* before "on the latter supposition." In reference to the question of *theoretic electricity*, I may further remark, that it is a solecism in philosophical language, as in logic, to use the term *negative electricity*. To *charge a jar with nothing*, (for the *negation* must mean here, as it does in common language, *an absolute privation*,) is incompatible with a correct mode of expression, and it happens that this *negation exhibits powers as active* and *positive* as its *antagonist*. We cannot attribute to a state of *rest*, the lively character of *motion*. If two powers be intimately blended together (rosin on sulphur and red lead), and projected by an elastic gum bottle against *a figure* described by *positive* electricity, on the *ball* connected with an electric sphere, *charged positively*, the *rosin* or *inflammable substance* will separate from the mass, and adhere to the *communicated electricity*; and if *negative electricity* is the agent employed, the *metallic oxide* will be the *subject of attraction*. These are experiments familiar to every electrician. In my opinion, the *same fluid* in *different quantities* should only attract *different portions* of the *same substance*.

I see

I see that Mr. Singer, in "the Elements of Electricity and Electro-chemistry," does not consider the propulsion of a pith ball by the electric discharge as any proof of the *direction* of the current; and I agree with Sir H. Davy respecting the *inclination of flame*, generally adduced as an evidence on the same side. Much therefore as I revere the opinions of Canton, Franklin, &c. I pay no *respect* to *great names* if *inconsistent with themselves*, and at *variance with truth*;—and as for the terms "oxygen and hydrogen electricity," they unfortunately do not apply. Surely the *more dispassionately* our reasoning is conducted, the more truly philosophical it is. I think Mr. Singer has much merit as an able practical electrician; and under this impression, I presume with due deference to remark, that though his application of the term *expansion may* be *conceived to apply* to the phænomenon where the *indents* are *directly opposed to each other*, it can *scarce be admitted* to weigh as *an argument* in my experiment, where *two or more wires* were used with *one above*, &c. I am at a loss to comprehend what is *meant* by "out of the circle" in the paper alluded to. *Perfect insulation*, like *absolute contact*, is not to be conceived. I think that Mr. Singer's plan of insulation is exceedingly ingenious, and founded upon just principles; because, by limiting the ambient conducting medium, he also confines the conducting power. The following *curious experiment*, which I made, presents phænomena not easily reconcileable with the Franklinian hypothesis. Having *coated the ball* connected with the *internal coating* of the *charged electric*, and *one of the balls* of the insulated *discharging rod, with China ink*, both being *uniformly obscured*, a *vertical card* was *placed between*. The *discharge* was *then made*. The paper as usual perforated a *circular portion* of the *China ink displaced* from *both balls*, and an *indent appeared in the centre of each.*

I confess I am astonished at Mr. Walker's mode of reasoning, in *his* "New Outlines of Chemical Philosophy." Chemistry, he seems to say, now, is what astronomy was in the days of Kepler. This appears to me, as if he had said, the *facts are sufficient*, but as yet *system exists not*. Now, I happen to think that *discovery* has not yet unfolded a *sufficient number of data*. Let experiment be diligently cultivated, or the gate leading to improvement will be for ever shut. None can *more sincerely lament* the *imperfection* of our *chemical nomenclature* than I do. I should however be sorry that it should be framed anew, by the insulated *effort of one*, and not by *the united labours* of *many competent* to a *task so arduous*.

In No. 189, page 22, Mr. Walker, after condemning terms in general use, and want of precision, adopts two new words "sanctioned

tioned by the terms Thermometer and Photometer now in common use." I shall only remark respecting Mr. Walker's "*Photogen*," that in the language of the Greeks (from whence the two words composing this term are derived) the literal interpretation is *to generate light*. How this applies to "the imponderable element of *combustibles*," I have to learn. In page 24, Mr. Walker observes "that light and *heat* are produced in the electric discharge. I observe *none* sufficient to *affect our most delicate thermometers*. I have by *this means suffered metallic laminæ* to be *fused* on the *back* and *ether inflamed* in the *hollow of the hand*, by a *spark taken through it*, by *means of a person uninsulated;* and surely, if sensible heat had been excited, I must have felt it. If none other more competent to the task than myself takes up the subject, I shall endeavour shortly to point out some inadvertencies into which I conceive Mr. Walker has fallen.

I am, with much respect, gentlemen,

Your obliged obedient servant,

J. MURRAY.

Messrs. Nicholson and Tilloch.

XXXIV. *On the Simplification of Mathematical Analyses:—a Paper read to the Liverpool Philosophical Society. By Mr.* EGERTON SMITH EYRES.

IN all inventive sciences, it happens that particular methods for determining every possible proposition cannot be laid down; for the science would in that case be no longer an inventive, but barely a substitutive one. But although we experience the impracticability of discerning rules so universal as to include every known case of the branch of knowledge with which they are connected, yet it is very possible, by proper explanations and concise methods, much to facilitate the operations and assist the invention; for the whole art of solution consists in reducing complex matter to a more simple form: and therefore, if we can determine wherein the complexity of problems consists, as opposed to their simplicity, we may know, not only when we have reduced a complex to a simpler problem, but also in some measure obtain rules concerning the method to be pursued in such reductions.

I think we may assume it as an axiom, that complexity increases with the number of data or things to be considered; that is, complexity is as the number of data directly; and, therefore, one division of the method of reduction will consist in diminishing the number of data from a greater to a lower number, till we arrive at the simplest possible conclusion, and that will be

the case when the number of data is reduced to one ; of less than which it is plain no problem can subsist.

There is also another source of complexity in mathematical propositions, and that is the *nature* of the data when their number remains the same; and this will in most cases be found dependent on the peculiar properties of the subject under consideration. But in geometry it has always some relation to the properties or habitudes of lines considered in a geometrical sense, as their *sum, difference, ratios, rectangles, squares, cubes,* &c. or the position they are in with respect to other lines known or unknown. It is not perhaps possible to say decidedly, that any one of these data is more complex than the others; only it seems natural to infer, that it is a simpler relation of lines, to consider them merely as regarding their length or ratios, than to draw the inferences necessary to solution from data concerning the squares or rectangles of lines, which latter combination may be conceived simpler than one which is connected with the cubes or solids. Moreover it is found that some relations of lines, whose habitudes or number of dimensions remain the same, are more difficult of determination than others, though apparently of equal simplicity: thus it is found that "Given the rectangle and *difference* of the squares of two lines to determine them," is a much more difficult problem than "The rectangle and sum of the squares." But we are not acquainted with any method of reasoning by which this difficulty might have been foretold; and are only able to estimate it by the nature and difficulty of the requisite analysis. There may however a reason be assigned for this difference of difficulty, and this in great measure depends upon the almost unaccountable prejudice that most mathematicians have in favour of the circle above all other curves, which prejudice induces them to reject all other modes of solving problems, that can by any possible, although intricate, analysis, be made to depend only upon the intersections of circles with circles, circles with right lines, or right lines with right lines. And so it happens, that when in any problem we can obtain the locus of a certain point connected with the determinations requisite to the solution of the problem supposed to be under consideration, that problem is then simplified in a degree proportionate to the value of the point whose locus is determined: but strict geometers confine this method of simplification to the cases in which the determined locus is a circle. And the only reason assigned for this rejection of other curves is, the greater relative difficulty of their description. This, as a matter merely *practically* considered, is certainly an objection of great weight; but in the theoretical determination of a problem, I cannot conceive why a prejudice of that nature should be indulged, at least to such an extent as

even

even to exclude the conchoid, a curve full as easy to describe as a circle.

From the very nature of things it follows, that some curves are more fitted to the solution of some problems than others, and for that reason it would seem equally rational, to require a mathematician to give solutions to the purely circular loci and problems, by means of the conic sections, or higher curves, as to fasten him down to the use of a circle and right line, in problems whose solution would be much simplified by the use of conic sections, or curves of the higher genera. It is impossible with the aid of all the known curves, excluding the circle, to apply a line of a given length, from a given point, to a curve of any order: therefore, in this case it plainly appears that the place of the circle cannot be supplied by any of the other curves; whence it follows that, strictly speaking, the circle is here a curve of the *highest* order, since it overcomes difficulties the others are unequal to. Whether this rejection of the other curves be well or ill founded, I may hereafter discuss, but shall for the present confine myself to problems requiring only the right line and circle. Now as it was before shown, that one part of the art of analysis consists in diminishing the number of data; therefore it follows, that we should first of all know how to apply the properties of our tools, which are here only two, a right line and circle. The application of those known properties, in a manner most suitable to the attainment of the desired end, in a great measure depends on having a perfect knowledge and experience of the nature of the geometrical loci, by means of whose proper application, one of the conditions of the given problem may be always so fulfilled, that there will not be any necessity of that condition occupying the attention of the geometrician any more; and so, if two loci be discovered, the problem is still further simplified.

Rules for Analysis.

The great art of obtaining geometrical solutions depends on a proper application of the geometric loci; therefore our first rule will direct their substitution whenever the nature of the data admits of such a simplification.

Rule 1. If you have either the sum or the difference of any two lines given, to determine them with the further assistance of some other given data,—A line must be taken equal to the given sum or difference, and the problem will be reduced to finding in that line a point, such, that the lines determined by that required point, and the two already known, may satisfy the conditions specified in the given data. But in case of the two lines proceeding from two given points to intersect under conditions that

do not allow them to form one continued right line, then the locus of their intersection is the curve of an ellipse or hyperbola, according as the sum or difference be given; which locus being described, simplifies the problem to one consisting of a less number of data.

Rule 2. If the ratio of any two lines proceeding from two known points be given with any other data, the construction of the locus is, by joining the two given points, and finding two others (one within and the other without), such that the ratio of the segments intercepted between the two given points may be equal to the given ratio; then describing a circle to pass through the two last found points, and whose centre shall be in the continuation of the right line passing through the given points:—that circle will be the required locus.

Rule 3. In case of the difference of the squares of two lines proceeding from two known points being given, to determine their locus, we connect the two points by a right line, in which we find a third point, such that the difference of the squares of the intercepted segments may be equal to the given difference of squares, and erecting a line from that point perpendicular to the beforementioned line, it will be the required locus.

Observation 1. If there be given the difference of squares, to which the squares of the lines under consideration have given ratios, the locus is obtained on the same principle by merely altering the situation of the given points, according to the ratio the squares bear to each other, on the principle of similar triangles.

Observation 2. If the lines in question do not proceed from two given points, but are tangents to two circles, whose centres and radii are known, the problem is solved in nearly the same manner as before shown, on the principle of a tangent being perpendicular to the radius, passing through the point of contact, and from that arguing upon the Pythagorean proposition.

Rule 4. But if the sum of the squares, along with any other data, be given, to determine the locus of lines proceeding from two given points,—Bisecting the right line that connects the two given points, and finding another line, such that double of its squares, together with double the square on half the given line, may be equal to the sum of the squares of the two lines whose locus is required; then describing a circle with a radius equal to the line just found, and whose centre shall be the middle of the line, joining the given points,—then will that circle be the locus that was required to be described.

This rule will, as well as the last, admit of two additional cases. 1. When there is given the sum of squares having given ratios to the squares of the lines intercepted between the two

given

given points and the point of their intersection with the required locus. 2d. When the two lines do not proceed from the two fixed points, but are tangents to two circles whose radii and centre are known. Both these cases will admit of circular loci, on the same principle as the similar cases adduced to Rule 3.

If you have the ratio of two right lines with other data, varying according to the circumstances of the case, there are a great number of loci that may be applied, according to the different forms the problem may assume. One of the most generally useful of that species of loci, is given in the second rule; but the following are some that apply to problems involving other conditions or data.

Rule 5. If it be required to determine the locus of one extremity of a line revolving round a point as a centre, and meeting a right line given in position when the ratio of the segments intersected by that line is a given one,—Draw any line from the fixed point to the line given by position, and continue it till the parts obtain the given ratio; then drawing through its extremity a line parallel to that first given, it will be the locus required, as will be evident by drawing any other line through the given point to meet the two parallels.

Rule 6. If it be required to determine the locus of one extremity of a line, which being parallel to a line given in position is always interposed between two other lines given in position, and continued, so that the ratio of the segments may be equal to the ratio of two known lines,—Draw any line parallel to the line given in position, and continue it so that the ratio of the segments may be equal to the ratio of the known lines; join the extremity of the line so continued, and the point of intersection of the two lines given by position; then will the line so drawn constitute the required locus.

Much more might undoubtedly have been added to the above rules on so important a subject as the geometrical loci; but as most of the higher and therefore more interesting geometry concerns the conic sections, &c. which are so much more difficult in their enumeration as hardly ever to admit of being described by mere words without diagrams, I have therefore here omitted them.

XXXV. *Notes and Observations on the Ninth* Chapter of Mr.* ROBERT BAKEWELL'S *"Introduction to Geology;"—embracing incidentally, several new Points of Geological Investigation and Theory. By Mr.* JOHN FAREY, *Sen., Mineral Surveyor.*

[Continued from p. 127.]

Notes, &c.

P. 208, l. 7, which fills dykes*.—* I have not observed in Mr. B's volume, any proper notice, of the curious and important Geological fact, that the skirts or sides of a fault, consisting of the ruptured edges of strata, generally (and I believe always in some degree) show a very considerable *wear and polish*, Williams's Min. Kin. 2d Ed. i. 13, P. M. xxviii. p. 120, and xxxiii. p. 258, Mont. Mag. xxviii. p. 463, Wern. Trans. i. 489, &c. Perhaps Mr. B. in speaking of the strata being often "shattered where they come in contact," (p. 147), may be alluding to this phænomenon?; see my Note on p. 209.

l. 7 and 8, is commonly indurated clay†.—† Rep. i. 500 and 501, and my 2d Letter, vol. xlii. p. 106.

l. 15, has seldom been explored‡.—‡ See p. 212, and my Note on p. 108, Rep. i. 146, 165, 200, 290, Note, Phil. Trans. 1811, and P. M. xxxix. p. 29, 95, and 101, my 2d Letter, vol. xlii. p. 106, &c.

209, l. 7, they descend is unknown*.—* It is equally unknown, from observation, to what depths *Faults* in general descend into the Earth, as with respect to Stone Veins, or *Dykes*; I have observed a phænomenon in the face of several Cliffs, Quarries, &c. which seems to indicate, that some of the Faults, *whose derangement is small*, do not descend a great way into the Earth. One of these occurred to me in the open-work for Ironstone, about 300 yards SE of Grass-hill Furnace in Hasland, (Rep. i. 397), as I was observing the same in November 1808, with William Anderson, the Foreman of the work; a fissure or small fault, eight or ten inches wide, filled with rubble and soil, was seen descending down from the surface, through the Binds and thin stone beds, for about ten feet, and there terminating, on an eight-inch strong stone bed, which was not in the least cracked or disturbed, or any of its under-measures; but the measures above, rose from this bed, towards the fault, and were supported by the rubbish therefrom that had run into the wedge-like openings, for four feet in length on the N side, and for 18 or 20 feet

* "*and the Eighth*" was omitted by mistake before *Chapter* in the last title.

on

[P.209] on the S side of the fault or derangement; whence the effects of *a lifting action* on these strata, was most evident, see my Note on page 50.

It was perhaps, to such shallow or superficial faults, that Mr. Williams alluded, under the name of *hitches*, in p. 23 of vol. i. of the 2d Edit. of his "Mineral Kingdom."

That all the *faults* whose derangements are considerable, must extend to vast depths in the Earth, no one can doubt, and to me it seems most probable, that the Earth is broken through to the antipodes, by an immense number of these fissures, in almost every direction, and that the separate pieces or piles of strata, after having for long periods *slidden by the side of each other*, in obeying the powerful Tidal actions that then prevailed (by which their surfaces became worn and polished, see my note on p. 208), at length came to rest in equilibrio, in their present positions, but from which positions, the parts of the Earth, thus only slightly adhering by their mutual attraction, are liable to be again moved, by any extraordinary Tidal action.

According to the Theories adopted by M. De Luc and some other Geologists, the *faults* terminated beneath, in vast subterranean *Caverns*, which formerly existed, as they imagine, but of which I think they have failed in producing the least proof: or of their adequacy, for producing the inequalities and frequent changes of strata, in position and kind, now observable on the surface of our Planet: but all of which complicated phænomena, appear to me, clearly explainable, by these general dislocations and tiltings of the strata, (which however, had *previously*, great inequalities in their planes and thicknesses), and the subsequent denudations, and the excavations of Valleys, &c. see Rep. i. 117, 123, &c.

The frequency of *faults*, in all districts where the Earth has been penetrated by Quarriers, Miners, Colliers, &c., and the absence hitherto, of any general knowledge or principles of this phænomenon, have occasioned the immense number of *Names* (amounting to near *eighty*) by which they have been called, see Rep. i. 118 N. Several of these, are very improper to be used by Geologists, because they have also, applications to other phænomena of the Earth; the term *Slip* is among this number, which notwithstanding its other and more proper application, to *modern subsidences* of masses of strata, (Rep. i. 73, 75, &c.) which have been partly undermined, or have *given way externally at their feet*, into a valley or other space, and have *left a cliff standing*, to

[P.209] mark such their modern subsidence, has been used by Mr. B. (P. M. xl. p. 46, and vol. xlii. p. 122) to denote *Faults*, which are fissures of very different kinds, both as to their period and manner of formation, as well as their mineral contents, &c. And although he has not, like M. Werner (New Theory of Veins, p. 61), completely confounded these very different phænomena, as observed, Rep. i. 74 Note, I have nowhere observed the subject of *slips*, or modern subsidences of tracts of ground, to be noticed in Mr. B's work.

l. 8, dykes which shoot up*.—* Crystallized masses, like the linings of veins, sometimes extend upwards into the Shale, Toadstone, or other stratum which covers the Rock usually containing the Vein, see p. 226, and Rep. i. 245; Gypsum masses in Red Marl, are often seen with projecting ribs almost like the comb of a cock, shooting out into the Marl; and Granite and other crystallized masses may in some instances, in this way "shoot up," for short distances, into the Rock which covers them.

With Basalt it may perhaps be otherwise, and in the instances, at the Clee-hills, mentioned by Mr. B. page 124, in Antrim, P. M. xxxv. p. 365, and in many others, where Whin-dykes intersect Basalt and its *under* strata, it may have been, that the fissures below existed, when the Basalt stratum began to be deposited, and that the same deposit descended into the fissure, instead of the matter of Whin-dykes being forced up from below." The vast stratum of Basalt, which I believe to have once covered all the Forth and Clyde Coal-field, before its astonishing denudations took place, may have occasioned by the descent of its first deposit, those Whin-dykes therein, which do not now connect with the Basaltic Hummocks remaining.

But there is another, and to me a more probable supposition, viz. that in the instances mentioned, of Whin-dykes intersecting strata, which lay *either above or below* Basaltic strata, the Dyke, in reality, passes through the Basalt as well as its upper and under strata, although its similarity in substance and structure, may require nice discriminations and well-conducted researches, to trace the Dyke through the Basalt, where the same *does not derange* or dislocate the strata: and the same of Granite or other Dykes, intersecting similar strata to their own substances. Where the Dyke *does derange the strata*, or there is a lift on one side or sink on the other side of it, there must I think be less hesitation in concluding, that the Dyke passes through its similar Rock, however indistinct the traces of it therein may be, than in adopting

[P.209] adopting the very unphilosophical supposition, ascribed to M. Werner by a late writer (F.) in Nicholson's Journal, vol. xxxvi. p. 161, viz. "Werner however, is of opinion, that in many instances the position of strata is ascribed erroneously to change (of level on the different sides of a dyke or fault), which in fact was the result of *their original formation*:—where changes actually *have occurred*, he ascribes them to ruptures," &c.—Still less can we admit with Mr. Kirwan (Geological Essays, p. 334), that the *Dykes existed in their present situations, before the strata on each side of them were formed!*, and that the filling up of one side of the dyke with materials for the strata, before the other, forced the dyke or retaining wall, out of its upright position, and thus occasioned the *hading* which so commonly attends Dykes ! ! See the article *Colliery* in Dr. Rees's Cyclopædia.

l. 23, charring the beds †.—† Query—see my Note on pages 108 and 125.

210, l. 17, are laid horizontally *.—* Dr. William Richardson, Trans. Roy. Ir. Acad. ix. and P. M. xxxv. p. 373.

211, l. 16 and 17, produced the irregularity*.—* Why may not a bump or thicker or higher part of the stratum beneath, have often occasioned rocks to rise abruptly, and dip in opposite directions ?: they have been found to do so in numerous instances, in Ashover, Crich, Dudley, &c. &c., see Mont. Mag. xxxiii. p. 516, and P. M. xxxix. p. 128.

And why may not *a* in fig. 1, in Plate I. be a case of denudated regular stratification ?, "the modesty of nature" being a little over-stepped, in Mr. B's drawing, see my Note on p. 61. Sir James Hall has lately described the effects of denudation on contorted strata of Killas, in the Edinburgh Trans.

l. 20, abrupt mountains †.—† Query, Rep. i. 123 Note, and my 2d Letter, vol. xlii. p. 106.

212, l. 3, action of currents *.—* These contribute now, materially, to the formation of cliffs on the sea-shores, P. M. xxxvi. p. 7.

l. 22 and 23, at Nottingham †.—† I have represented the great Derbyshire Fault as "commencing near Nottingham," (Mr. B's, line 7) *in the Red Marl*, or as beginning only on the west of that place, to derange the measures, and as increasing rather rapidly in its effects thence westward, to Allestry, &c. : about Nottingham, therefore, the effects might be expected to be the less visible, than in any other part of its course, by one acquainted with my theory of Faults in Rep. i. 117: to Mr. B's objections on this head, other replies will be found in my 2d Letter, vol. xlii. p. 104.

P. 213, l. 23, from the conjectures *.—* Are not the "*conjectures*," in the work alluded to, always distinguished *as such?*, and the precise localities, if not the full circumstances, of all the *facts* stated, and names of authorities given?:—on which points, Mr. B. might have taken a useful lesson, for his work.

214, l. 11 and 12, filled up the fissure *.—* Rep. i. 247.

l. 18, crystalline forms are obtained †.—† Rep. i. 247.

l. 24, narrower in their descent ‡.—‡ Rep. i. 251.

215, l. 14, Ecton copper-mine *.—* Rep. i. 258 and 353.

216, l. 22, fluor spar mine*.—* Mawe's Min. of Derb. p. 69; Water-hull, Cliff-side, and Old-tor Mines, Rep. i. 269 and 461.

217, l. 7, the natural caverns *.—* The remarkable property of thick *Limestone Rocks*, in every part of the world, I believe, to produce *Caverns*, is not noticed in this work, or this geological phænomenon mentioned, except here and at p. 18, I believe, and when they occur as wide places in Veins, at pages 214 and 216; although it should seem, from your account of Mr. B's Lectures in vol. xxxix. p. 236, that he there particularly noticed them, and attempted to account for their formation, in a manner, to which I opposed some facts (in page 427 of the same volume), to which he has not replied at p. 47 of vol. xl. but this common and important phænomenon, is almost excluded from his subsequent work.

That the *shrinking* or contraction of the masses of Limestone, have occasioned the Caverns and large cracks, which *so remarkably distinguish calcareous Rocks*, can admit of no doubt, as observed in my Rep. i. 292; and the same principle will doubtless account, for nearly all known natural *Caverns*, I believe, as well as for the opening of *Mineral Veins*, in general, which I have more effectually shown, I believe, (Rep. i. 246, P. M. xxxix. p. 428, &c.) than any other English writer, and yet, at p. 223, Mr. B. persists in ascribing to M. Werner, *only this mode*, viz. of *shrinking*, for opening of Veins; notwithstanding his own express reference to *Slips*, (as observed Rep. i. 74 Note) and more particularly, to the splitting and falling asunder of Rocks *by their own weight*, by M. Werner himself, at pages 49, 50, 82, 88, 89, 95, &c. of the translation of his "New theory of Veins," see my Notes on pages 299 and 223.

218, l. 3, and unites again*.—* It seems doubtful, whether Mr. B. here alludes to what are called *Riders*, those large stony masses, which are commonly found in Veins, and which were until lately, said to be fragments rent off from the adjacent Rocks; and many fine theories were accordingly

[P.218]ingly invented, to account for *their suspension in the vein without touching the skirts* or Rock: on all these Mr. B. is silent. In my Rep. i. 248, I have, I think, clearly shown, that *Riders* do not belong to the adjacent Rock, or to any other, but were formed where they are, by a rudely and confused crystallization, *since the matter of the vein*, in Tick-holes or Druses, that were previously empty.

l. 5, another vein †.—† A Fault-vein, or Vein intersected longitudinally by a *fault*, Rep. i. 249, 245 Note, &c.

220, l. 19, its two extremities*.—* Tideslow-Rake, see Rep. i. 268;—which account of mine, needs correction, in consequence of Mr. Elias Hall's subsequent investigations (mentioned in my 2d Letter, vol. xlii. p. 113,) by adding, Peak-Forest, Small-dale and Dove-hole, to the names of Liberties through, or near which it ranges, according to Mr. Hall's Model, now before me; and adding, 3rd and 4th Limestone, to the Rocks which it intersects. The great Limestone Fault crosses it (and perhaps terminates its works to the W), N or NW of Dove-hole, and a branch therefrom, crosses and breaks its vein-stuff, in High or Hills Rake, near Windmill-houses.

This Vein, crossing the entire series and limestone district, and passing unusually far under the shale to the E, is perhaps the most extraordinary in Derbyshire: and respecting which, I sincerely regret, from mistaken information, as well as observation, to have very improperly expressed myself, at the top of page 275 of my Report.

221, l. 23, the year 1803 or 1804 *.—* These dates (see also p. 310), as to when carbonate of Lead or *White Ore* (Rep. i. 355) first *became known* to the miners of Derbyshire, are somewhat incorrect; since Mr. John Mawe, in his "Mineralogy of Derbyshire," published in 1802, says, that it had been then known by them, a "few years," p. 106 and 370.

223, l. 1 and 2, vegetable fibres, apparently *roots**.—* I wish that Mr. B. had informed us, of the particular Colliery and situation of the fault-stuff, in which he made this curious observation. In the clunch or fire-clay floors of the Coal-seams, (see my Note on page 161) in Castlecomer and Feroda Collieries in Kilkenny in Ireland, fibres, like roots, are said to be commonly found, see Mr. William Tighe's "statistical observations on Kilkenny," p. 56, &c.

When examining the Quarries of blue Lias Limestone at Barrow-on-Soar, in August 1807, the workmen had just uncovered a considerable area, of what they called the Rummel floor, a useless bed of blue slaty Limestone, 10 inches thick,

[P.223] thick, lieing at a considerable depth from the surface; and on my asking, which were the beds in which they usually found Shells; one of them successively struck the point of his pick into the floor of stone, on which we were standing, in places that at first sight presented no unusual appearance, and thereby turned up several plano-convex lenses of large size and very regular form, out of the stone floor, which lenticular masses, he called shells and skelps: on examination, they proved to be almost entirely composed of cornu-ammonii, (curiously covered by branching spar, which appeared compressed), and of other shells: but what occasions my mentioning the same here, is, the appearance of numerous fibres, exactly like small *roots*, crossing each other in the manner of a net, which lined the bottom of all these lenticular cavities in the Limestone.

It might be important, for those who have the opportunity of repeating these observations, to ascertain, whether these fibres are of vegetable or of animal origin: it seemed very unlikely to me, that they could have recently originated from plants on the surface; but this ought to be well examined, at the time of removing the stratified clay (called Rummel carf) and other matters, from off this bed of stone. The Roots of Coltsfoot, Sainfoin and some other plants, are said to descend very deep into the fissures of Limestone and other Rocks: but not through strata of solid Clay, I should think.

l. 5, by the shrinking†.—† Rep. i. 246. The zealous disciple of M. Werner, who in 1809 translated his "New theory of Veins," with *Notes* of his own, did not seem to be aware, nor has it since been stated, I believe, that *the other causes*, so *principally* insisted on for *the opening of veins*, have been withdrawn by M. Werner, and *this only*, the "shrinking of the materials," fixed on; which had been only casually, and indeed improbably hinted at, as connected with drying and with earthquakes, as I have mentioned, Rep. i. 74, Note; see my Notes on pages 209 and 217.

A very recent writer, who under the signature of F. in Nicholson's Journal, vol. xxxvi., gives a general view of the Geological System of M. Werner; at p. 161, thus mentions that Geognost's ideas, of the manner in which ruptures of the strata and fissures were occasioned, viz. "by the unequal accumulation of rocky matter, at the time of deposition, by *the loss of support*, owing to the diminution of the Waters," &c.

l. 12, repugnant to facts ‡.—‡ Rep. i. 246, P. M. xxxix. p. 428.

P. 224,

P. 224, l. 14, bearing measures *.—* Rep. i. 246, P. M. xxxix. p. 428.

l. 22 and 23, the round pebbles †.—† Rep. i. 249.

225, l. 3, contributed to the effect *.—* At the conclusion of his work on Veins, M. Werner particularly recommends the Mines of Konigsberg (transl. p. 53), and of the Peak of Derbyshire (p. 130), where these anomalies were there stated to occur, to the careful study of Mineralogists. Mr. B., though a native of the latter district, (P. M. xlii. p. 123,) may perhaps hereafter be thought, to have added but little to the knowledge thereof.

l. 13, ceases to contain any ore †.—† Similar facts, I believe, with regard to Mines in the north-west of Yorkshire, or in Durham, occurred to Mr. John Hutchinson, more than a century ago, and were in 1749 *published* in his Works, vol. xii. p. 253, which were unknown to me, until very lately, Rep. i. 245. It has too commonly been found, that the Veins in the lower part of the 1st Limestone became dead, or contained spars only, considerably before the Miners reached the 1st Toadstone, in sinking in these Veins.

l. 20, but in very small quantities ‡.—‡ See Rep. i. 250: in Gang Mine, (Rep. i. 258) the short branches of veins in the 1st Toadstone, were so very productive, that the late Mr. Joshua Gregory, the overseer, (see my Note on p. 226) has assured me, that not less than 1000*l.* worth of ore was obtained therefrom ! !

226, l. 7 and 8, sand-stone *.—* Limestone *Shale*, see my Note on p. 93.

l. 24, seams of clay †.—† The *thickest* of these way-boards, *sometimes* divide the Veins, according to the information of Mr. Joshua Gregory, Rep. i. 245, and my 1st Letter, vol. xlii. p. 58 Note.—Since the same was written, I lament to have read of the death of this able and truly respectable individual, (Mont. Mag. xxxv. p. 501).

Mr. Gregory had came to town, in the beginning of May, on the businesses of the *Cromford* and *Mearbrook* Soughs, (Rep. i. 329 and 330), and the *Gang* Mine, of which last he was the Manager; but finding himself very unwell, he set off rather suddenly, on his return home; which he never reached !, but after an illness of some duration, died at Market-Street, in Hertfordshire, aged 60 years. His loss, to the proprietors, of the most considerable Mine at present in Derbyshire, will be most severely felt, as well as by his relatives and friends; and by none is his loss more sincerely lamented, than myself.

[P.226] l. 18 and 19, states of electricity ‡.—‡ I wish, in following up this very ingenious idea, (P. M. xxxix. p. 428), that Mr. B. would consider and examine, how far the Voltaic influence seems to vary in its effects, in the *fault-veins*, (see my note on p. 218), owing to *different* Rocks, or beds of the same Rock, *being opposite to each other*, instead of the cheeks being similar, as is usual: as also, that he will apply the same considerations, to *faults*, near to the *Hot springs* (so general in Derbyshire), which bring strata beneath, into contact, or nearly, which were not originally so, see my Notes on pages 304 and 306.

[To be continued.]

XXXVI. *Memoir upon the Employment of Oxygen Gas in various Cases of suspended Animation. By M.* Sementini*.

The object of this memoir is to suggest a speedy and efficacious method of contributing to the processes at present in use, for the restoration to life of drowned or suffocated persons.

What is meant by suspended animation, may be understood by referring to the cessation of the motion of a pendulum, merely from a mechanical stoppage of its oscillation, while none of the parts of the machine have been injured. Now when there is a want of sensation and motion caused by an absolute suspension of the same faculty, we may expect the return of life by giving it such an action that it may resume its natural state, as the pendulum resumes its functions when a mechanical impulse has restored the oscillation which is peculiar to it.

But the animal organization is such, that the state of suspended animation speedily becomes dangerous, and apparent death is soon changed into the réality, on account of the facility with which the humours of an animal body are changed.

Now suspended animation may be produced by causes which act either on the general system, or directly upon the organ of respiration; but I intend to allude here to those cases only which, deriving their origin from any given cause, have instantly produced such a derangement in the functions of the lungs as to produce apparent death.

Recently, when directing my attention to the contrivances called fumigatory boxes, it occurred to me, that since inflation was generally regarded as the most energetic method of restoring animation, oxygen gas might be employed instead of common air. I afterwards became acquainted with the work of Dr.

* *Annales de Chimie*, tome lxxxvi. p. 140.

Goodwin,

Goodwin, published at London in 1788*, and I found that this author had tried the oxygen gas administered by inflation to animals drowned on purpose, and that he had fully ascertained its efficacy. I repeated his experiments with success, and they added confidence to my own previous opinions.

But the case of an animal drowned for the express purpose of an experiment, and which had been saved by the inflation of oxygen gas previously prepared at leisure, is very different from that of a man who has been drowned by an unforeseen accident. In the former case, every thing is arranged for the experiment, and the oxygen gas is ready beforehand; but if it be requisite to afford speedy assistance to a human being just taken out of the water, too much expedition cannot be used in preparing and administering the gas. It is the object of this short paper, therefore, to exhibit the method which I have contrived for introducing oxygen gas into the lungs instantaneously, and while they are yet warm.

For this purpose I had recourse to Berthollet's important discovery of the hyperoxygenated muriate of potash, which, among other wonderful properties, possesses that of containing nearly a third of its weight of oxygen, which, when exposed to a moderate heat, is reduced to a state of gas; and upon this remarkable property my process is founded.

If inflation, therefore, be the principal resource in cases of suspended animation, if oxygen gas be preferable to atmospheric air, and if the apparatus which I am about to describe be proper for developing the oxygen gas in a very short time, and for introducing it instantly into the lungs, I shall not have uselessly directed my humble talents to the welfare of society.

Description of the Apparatus.

In fig. 1. (Plate III.) is seen a cylinder of wood A, with lead at bottom to keep it steady. Into this is screwed the neck of the brass retort B. To the foot of this cylinder a spirit lamp is fixed, the flame from which embraces the belly of the retort. To the cylinder a flexible leather tube, *ddd*, is attached, which at the other extremity unites at F with the bellows E. These bellows terminate at G with a portion of a tube of elastic gum, which is furnished with a small brass plate to fit the human mouth. The bellows are provided at F with a valve†, and the inflation of the gas is effected as follows: The cylinder A is placed on a small table by the side of the bed on which the patient re-

* "The Connection of Life with Respiration; or, An experimental Inquiry into the Effect of Submersion."

† This valve might perhaps be attached as efficaciously to the usual aperture in the under side of all bellows.—EDITORS.

clines

clines horizontally: the *hyper-oxygenated muriate of potash* is introduced into the retort B, and it is screwed to the cylinder: the lamp is lighted, and the bellows E are applied to the mouth of the patient, while an assistant holds the nostrils firmly with his fingers. The oxygen gas begins to be developed; and as in the interior of the cylinder the aperture of the retort communicates with that of the tube *d d d*, this gas, not having any other vent, proceeds into the bellows, which are kept open on purpose. When the bellows are supposed to be full, or nearly so, they are shut; and the gas, not having any way of escaping, from the valve being shut, is compelled to enter into the patient's mouth and lungs. This process, which may be repeated at intervals, along with the other methods already known, forms the essential part of the very simple method which I propose. I have had the pleasure, with the above apparatus, to restore animation to one person, who was taken out of the sea, and supposed to be drowned.

XXXVII. *Extract from a Memoir upon the Existence of Alcohol in Wine. By M.* Gay Lussac. *Read at the French Institute, March* 1, 1813*.

It was my object to determine more precisely than has hitherto been done, the true epoch at which alcohol is formed in wine. Is it formed during distillation, as many chemists suppose from the experiments of M. Fabroni†, or at the moment of fermentation, as Mr. Brande thinks, without however giving any thing like satisfactory proofs?

It will be recollected that M. Fabroni established his opinion, by demonstrating from his experiments that alcohol could not be extracted from wine when saturated with subcarbonate of potash; whereas, by the same method, the smallest quantity of alcohol which might have been added to the wine was recovered.

Mr. Brande proves incontestably‡, that we cannot extract alcohol from wine by M. Fabroni's process; but he does not destroy his opinion, without showing that we constantly obtain the same quantity of alcohol from wine by distilling it at the varied temperatures of 93°,3, 87°,7, and 82°,2. It is nevertheless evident that the temperature 82°,2 is still high enough for the formation of the alcohol during distillation: hence it follows, that M. Fabroni's opinion is not completely overturned—far less is that of Mr. Brande very clearly established.

I have proved that subcarbonate of potash may be used to de-

* *Annales de Chimie*, tome lxxxvi. p. 175. † Ibid. tome xxx. p. 220.
‡ Phil. Trans. 1811, p. 337. Vide Phil. Mag. vol. xxxviii.

monstrate

monstrate the presence of alcohol in wine; but we must begin by isolating the foreign substances which are mixed or combined with it, and which oppose its separation. The process which I consider as the most advantageous for this purpose, consists in shaking the wine with well pounded litharge: it speedily becomes limpid like water, by giving to the litharge its colouring and extractive matter: the subcarbonate of potash then very easily demonstrates the presence of alcohol.

I can give another proof of the existence of alcohol in wine, equally conclusive, by distilling wine in vacuum at the temperature of 15°, which, as is well known, is very inferior to that which is developed during fermentation, for I obtain a product decidedly alcoholic.

These two experiments prove beyond doubt that alcohol is not formed during fermentation, as was generally supposed previously to M. Fabroni's experiments.

I shall terminate my memoir, which is about to appear in the third volume of the *Mémoires d'Arcueil,* by showing that we may obtain the pure alcohol of Richter by employing quicklime, or rather barytes, in place of muriate of lime.

XXXVIII. *Experiments tending to prove, that neither Sir* Isaac Newton, Herschel, *nor any other Person, ever decomposed incident or impingent Light into the prismatic Colours.* By Joseph Reade, *M.D.*

Cork, January 24, 1814.

Sirs,—Sir Isaac Newton, for the purpose of decomposing light, made a small hole in his window-shutter a quarter of an inch in diameter, and, having placed a prism so as to refract and receive a spectrum on a sheet of white paper, perceived seven colours in the following order: red, orange, yellow, green, blue, indigo, and violet: these he supposed to be primary colours, which when combined in certain proportions gave white or transparent lights. The necessary shortness of a letter will not permit me to enumerate his experiments; I therefore refer to his Optics. That this philosopher was mistaken in supposing he analysed incident light, will appear evident from the following experiments and observations. When we look with a prism at a window, the light passes through the panes, and likewise through the prism, to the eye, undecomposed, and consequently colourless; but when we look to the frames, we perceive an artificial rainbow of reflected blue, red, and yellow. Any opaque substance, as a piece of black cloth or paper, when pasted on the window, will produce the same effect; and the more dense or dark, the deeper the tints or fringe*.

* I use the word *fringe* as much more appropriate than *penumbra,* whose derivation is absurd.

 The

The north or top of the paper will be fringed with blue; the south or bottom, with red and yellow rays. Now it is evident, if light were decomposed by merely passing through the prism, according to the different refrangibilities of its coloured rays, that light admitted through the panes should be equally decomposed with that in the vicinity of the opaque frames. To place this objection in a stronger point of view, I made the following experiment.

I cut two holes in my window-shutter, one the diameter of a quarter of an inch, mentioned by Sir Isaac Newton, the other the diameter of four inches; and having darkened the room, and applied a prism, I found that the small aperture admitted light tinged with the seven colours, which I could receive on a sheet of white paper: the larger orifice was also fringed round with seven prismatic colours, and pencils of white light passed through the centre.

Here I must again observe, if white incident light were decomposed by merely passing through the prism, why was not that coming through the centre equally decomposed with that at the edges? And however contrary to received opinion, I am confident it is nevertheless true, that incident light has never yet been decomposed, but that all experiments hitherto made have been on light condensed and reflected by opaque substances. If we paste a piece of black cloth on the window, whose colour, as I have shown in my last communication on blackness, arises from the reflection of condensed rays of blue, red, and yellow; on applying the prism, a fringe of red and yellow appears at the south. This does not proceed from a decomposition of incident light striking on the edges of the cloth, but it proceeds from an actual decomposition of the condensed coloured rays of the black cloth itself. The prism decomposes these three primary colours according to the order of their different refrangibilities; and as the red and yellow rays are more refrangible than the blue, as I shall show in my next communication, they are brought down by the prism, and the black cloth remains of a blue colour. The further we move from the window, the more refrangible the red and yellow rays become, and consequently the decomposition is the greater. In this experiment the north of the cloth reflects blue rays, the south red and yellow, proving in the most satisfactory manner that there are but three primary colours; and as all the secondary or mixed colours can be formed of blue, red, and yellow, to call others into existence would be contrary to the beautiful simplicity of nature, and unnecessary. But it might be asked, If there are but three primary colours, how did Sir Isaac Newton produce a spectrum of seven?

The following experiment will explain: Paste a strip of black cloth or paper, six inches by three, on the window; on the south you

you perceive a fringe of reflected red and yellow: paste another similar strip parallel to this, at about four inches distance, on looking through the prism you perceive the north to be fringed with blue. Thus we have three primary colours nearly in contact. The yellow rays of the upper paper, being the most refrangible, come nearest to the blue of the lower paper; and if we approach them, a green is formed by their mixture, so that we can now without any difficulty account for five of Sir Isaac Newton's colours, red, orange, yellow, green, and blue. By making a small hole in his window-shutter, he brought the northern and southern fringes into contact or mixture, and produced five colours with three. It now remains to account for the indigo and violet; and here I must again refer my reader to my last communication, in which I have shown that blackness arises from the reflection of blue, red, and yellow; which being granted, the solution of this otherwise difficult question becomes easy. The red and yellow of the lower cloth or paper, being more refrangible than the blue, were brought down by passing through the prism, leaving the upper part of the lower edge (when illuminated by the undecomposed light coming through) blue. Under the blue appeared indigo, which, as I shall hereafter show, is composed of blue, red, and yellow, in a different state of condensation from black. And at the bottom of all appeared violet, arising from a great quantity of yellow and red, which had been brought down, mixed with the black rays. From this experiment we might conclude, that Sir Isaac Newton by mixing three primary colours made seven.

But I am aware, it might be objected, that Sir H. Englefield and others decomposed, or thought they decomposed, incident light coming immediately from the sun, by passing it through a prism placed at an open window. So far however from refuting, this experiment confirms my opinion, that incident light was never yet decomposed, as I shall now endeavour to prove. The prism, being a semitransparent substance, when turned in such a manner on its axis as partly to reflect and partly to transmit the rays of light, (for it will never decompose if turned at right angles to the sun,) condenses and reflects fringes of blue, red, and yellow, from each of its angles; and these fringes of reflected light, being carried forward through the prismatic planes by the incident and undecomposed light, intermix by their different refrangibilities, and form a spectrum of seven colours; and as there are three angles in every prism, so there are two spectra always formed, in the same manner as three strips of paper, pasted parallel to one another on the window, will also form two spectra. As I am well aware that my experiments and opinions are in opposition to authorities of the first respectability in science; and

as I am also certain that science and liberality always go hand in hand, I rest my ideas on experimental inquiry.

To show that the decomposition of light takes place only at the prismatic angles, and arises entirely from those fringes of reflected light, I made the following experiment: When the sun was shining very powerfully, I placed my prism on a table at the open window; and having formed a spectrum on a sheet of white paper, I slowly turned the instrument on its axis, until I separated the red and yellow from the blue, and in place of green, white or undecomposed light passed through between the angles. I now ascertained that the red and yellow rays passed through the upper and thin angular edge, by intercepting them with my finger placed on it; and by running my finger along the middle angle, I intercepted the blue rays; and by pasting a narrow strip of paper between those two angles I made two spectra. But to place the fact beyond the possibility of doubt, standing at a little distance, I looked by means of another prism at the light passing through, and perceived three beautiful fringes hanging from the angles. Indeed it is surprising that those fringes, so easily proved, so evident to the eye, and so highly important in their consequences, should have escaped the observation of such able and accurate experimenters as those already mentioned. I shall conclude this paper with the following deductions.

1st. That incident light has never yet been decomposed; and that Sir Isaac Newton and other philosophers only decomposed light reflected from opaque substances, or fringes of blue, red, and yellow.

2dly. That there are but three primary colours, blue, red, and yellow, by the mixture of which, either by the prism or the painter, all the others are formed.

3dly. That Herschel, Leslie, Davy, Englefield, and other philosophers, drew their conclusions, relative to the heating power of the prismatic colours, from erroneous data, viz. from experiments on reflected light, whose heat must in a great measure depend on the reflecting media, and also on the thickness or thinness of those parts of the prism through which the fringes pass. Thus the red and yellow rays passing through the very thin upper angle, must be accompanied by more radiant caloric than the blue rays, which pass through the thickest.

The following diagram will demonstrate my opinions; and as I am at present engaged in a series of experiments, to prove that the prismatic colours have similar heating powers, I shall not anticipate.

Let *s* represent the sun (fig. 3. Plate III.) *d. g. f.* rays of undecomposed light, impinging on the angles A. B. C of the prism. These

These carry forward the angular fringes formed by reflection, which being refracted towards the perpendicular fall on the spectrum H. The red and yellow rays, passing through the thin angle A, must be more heated, when falling on the spectrum H, than the blue rays passing through the angles B. C.

Sir, I beg leave to remain your obedient servant,

JOSEPH READE, M.D.

*** In the communication, volume xlii. p. 418, Dr. READE's name was printed READER, which our friends are requested to correct with a pen.—EDIT.

XXXIX. *Observations on Colours, as applicable to the Purposes of the Artist. By Mr.* THOMAS HARGREAVES.

Liverpool, Dec. 12, 1813.

SIRS,—I TAKE the liberty of addressing to you some observation on colours, the result of great attention to the subject, and of long experience as an artist, which may perhaps be useful for the purpose suggested by Mr. Forster, in his communications inserted in your last and the preceding numbers.

I agree with him in the theory of three primary colours only, and with his account of the effects of the binary and ternary compounds; but his idea of the precise tint of one of those primary colours is certainly incorrect, from his having adopted the prismatic scale, which is defective, as I trust will be evident in the course of the following remarks.

Before I proceed, I think the annexed figure (fig. 2. Plate III.) will assist me in my explanations. In this figure the three simple primary colours, red, yellow, and blue, are placed at the three angles of a triangle, and the three intermediate colours, orange, green, and purple, in their proper situations between the respective colours which compose them, and at the angles of another triangle. The space between a primary and intermediate compound is likewise marked by the two letters on each side: thus, between yellow and orange is put *yo*, to signify yellow orange, or an orange colour approaching to yellow. It is evident that the simple colours are like mathematical points, neither extending one way nor the other; but an intermediate colour has an infinite variety of tint, from the verge of one of its components to that of the other. White and black, with all the intermediate degrees of gray, are equal combinations of the three simple colours. Pure white, by uniting all the colours, can only be produced with the rays of light, as shown by the prism; but all the different degrees of broken white or gray, down to black, may be produced by the coloured substances used in painting. An equal

 portion

portion of two simple colours makes the exact intermediate one, as of yellow and blue makes a green; but another equal portion of the third simple colour, as red, neutralizes the whole, making it a gray, light or dark, according as the colours employed are light or dark. If only a small quantity of the third colour be used, it breaks the tint, making it approach more to a neutral tint, according to the quantity used. By neutral tint, I do not mean the tint so called by landscape painters, which is inclined to blue and sometimes to purple, and ought rather to be called an aërial tint; but I mean by it, that negative gradation between white and black, which is best given by Indian ink.

A mixture of two intermediate colours will produce a broken tint of the colour which lies between them in the figure; for instance, green and orange will produce a broken yellow; green and purple, a broken blue; and purple and orange, a broken red; as may be accounted for thus: Green is one part blue and one part yellow, orange is one part yellow and one part red: thus, in the whole, when mixed together, there are two parts yellow, one part red, and one part blue: the red and blue and one part of the yellow would produce a neutral tint; but the additional portion of yellow gives a tinge to the whole, and produces a broken or brown yellow.

The effect of the three intermediate colours mixed together will be the same as the three simple ones, that is, they will produce a gray or neutral tint of a depth proportionate to the strength of the colours employed, which will be evident on considering them. It will be unnecessary to say further, that equal portions of all the colours, simple and intermediate, will still produce a neutral tint, as will be evident from what is shown already.

The figure shows at once what colour or tint will neutralise another, they being directly opposite in the circle. Thus, opposite to yellow stands purple, which used in a proportion of two to one will neutralise the yellow; opposite to green stands red, which will neutralise one the other; and so on with the whole circle. I have hitherto considered only those proportions in the mixtures which produce a neutral colour; but by varying the quantities of the opposite or neutralising tint, when mixed with the different colours in the circle, all the variety of what are called ternary compounds may be produced: thus, a small portion of blue mixed with orange produces brown orange, the tint of which may be varied by more or less of the blue into a great number of different shades, all of which will still belong to the orange so long as that colour predominates. If the next perceptible variation from orange towards red be taken and mixed

with

with its opposite in the same way, it will produce as many varieties of gradations; and by going thus round the whole circle, every tint that can be seen in nature, or imagined, will be formed.

As it is of consequence that the peculiar tint of each of the three primary colours should be determined, I will endeavour to communicate my ideas of each of them by reference to flowers, or more particularly to those pigments used in (water colour) painting which approach the nearest to them.

With respect to the primary colour of yellow, there is less uncertainty in the general application of the term than with either of the others; for the least tinge of orange readily procures it the name of orange or golden colour, and so likewise the slightest tinge of green gives it the name of green. This colour is procured in all its different degrees of strength from gamboge.

Blue, the name of the second primary colour, is applied with much less precision; for we find that it is given to many of the gradations towards purple, and likewise when it falls into a greenish hue. From an attentive consideration of the colours of flowers, I have remarked that the most usual tints are different gradations of orange, purple, and red. The primary tint of yellow is seldom seen pure, and the blue still less, scarcely ever; the pigment approaching the nearest to which is Antwerp blue. Ultramarine and Prussian blue tend more or less to the purple.

I come now to the third primary, red; and here we find the greatest uncertainty, as the term is given to all the shades of orange. This error is particularly adopted in the Newtonian theory. This arises from its being founded on the prismatic experiments: in the prismatic spectrum the third primary colour is not shown, the two ends of the spectrum are equally near to it: if there be any difference, I think the violet is more inclined to it than the other. In the figure, the colour which falls under the denomination of orange is precisely that which is called red in the prismatic spectrum: its tint is between that of vermilion and red lead, and is of course, from what has been said, exactly neutralised by blue. In the same manner, the colour called purple in the figure is nearly the same with the prismatic violet, and is neutralised by yellow. This will readily be proved likewise with the prism; for, if the spectrum is thrown on a strong blue ground, the colour which is there called red will disappear; and if on a pure yellow ground, the violet will be neutralised.

The true primary colour of red is that which is called crimson: this colour in its various degrees of strength is very frequent in flowers; the rose gives it in its paler gradations; but the pink or carnation shows it in all its gradations from the palest to the most deep. This colour is most nearly given by carmine.

With gamboge, Antwerp blue, and carmine, an artist can most certainly

certainly imitate every tint in nature; with gamboge and carmine the prismatic red, and all the other gradations of orange, may be made: and carmine with Antwerp blue gives the prismatic violet, indigo, &c. But if instead of carmine he takes vermilion, or any other pigment of the hue of the prismatic red, he will find it impossible by joining it with blue to produce crimson, pink, violet, or any of the purples.

On looking through the prism, or a convex lens, objects are tinged on their edges with a fringe of colour; according to the relative positions of light or dark, that fringe is either yellow running into prismatic red, or blue running into violet. These two fringes united in the prismatic spectrum form the whole range of colours there shown. But, from what I have said, I think it is evident that it is deficient one third of the circle, which third includes the true primary colour of red with its most immediate compounds.

I am unpractised in writing; but if I have advanced any thing that may be useful, in language that may be understood, I hope that any defect of style will be treated with indulgence.

I am, sir, yours respectfully,

THOMAS HARGREAVES.

P.S.—Is Mr. Sowerby's work on Colours published?*

To the Editors of the Phil. Mag.

XL. *On the pretended Formation of Oxalic Acid in a Mixture of Alcohol and Sulphuric Acid.* By M. VOGEL†.

A CONSIDERABLE time has elapsed since M. Cadet de Gassicourt announced, that oxalic acid could be formed by the simple contact of sulphuric acid and alcohol. The author expresses himself in the following manner‡: "I made a mixture of equal weights of well rectified alcohol and sulphuric acid: this mixture remained at rest for about thirty hours. At the end of this period I perceived at the bottom of the flask, and attached to the lower sides, a quantity of crystals about ten or twelve millimetres long, and formed by tetrahedral prisms terminated by a dihedral summit. After having decanted the liquor, I examined these crystals; and I ascertained, with the help of calcareous salts and reagents, that it was oxalic acid." M. Cadet then promises to resume this experiment on a large scale; which, so far as I know, he has not yet done.

This very remarkable result of the formation of oxalic acid by means of the sulphuric acid, has been nevertheless quoted in a great

* Yes; and may be had of the ingenious Author, No. 2, Mead Place, Lambeth.

† *Annales de Chimie*, tome lxxxvi. p. 219.   ‡ Ibid. xxxv. p. 200.

great number of works, and no person has hitherto called the fact in question.

Having occasion annually to make a quantity of sulphuric ether, I have always remarked that the mixture of equal parts of alcohol and sulphuric acid becomes milky, and deposits a white crystalline powder.

With a view to collect this crystalline deposit, I mixed the liquids in a large flask, and I decanted it after a few days in order to proceed to distillation.

After having preserved the precipitates for several years, I mixed them together in order to subject them to an ulterior examination.

This powder, when mixed with some fine needle-formed crystals, being dried with a gentle heat, had an acid taste, owing to a little sulphuric acid which was interposed, and which I took up by washing with alcohol. This alcoholic liquor contains merely, as I have said, sulphuric acid, and not a trace of oxalic acid. The powder when thus washed was perfectly neutral: it did not even contain any substance of the organic kingdom; for, when heated in a retort, it is not carbonised, and no gas is extricated. Boiling water dissolves a small quantity, and this solution is turbid with oxalate of ammonia, as well as by the muriate of barytes. The white residue, insoluble in boiling water, immediately becomes black on placing it in contact with sulphuretted hydrogen, or rather with the hydrosulphuret of ammonia.

I pass over other details of experiments; for it is easily seen that the crystals, which are deposited from a mixture of equal parts of sulphuric acid and alcohol, are nothing else than a compound of sulphate of lime and sulphate of lead*.

XLI. *The Propagation of Sound, according to the Newtonian Theory, demonstrated;—with Remarks on the one advanced by* LA PLACE, *and other Observations. By* RICHARD WINTER.

Whitby, March 15, 1814.

SIRS, THE theory of the propagation of sound through the atmosphere, is a problem that has engaged the attention of mathematicians and philosophers of the first celebrity; but the great differences deduced from the most accurate observations of experimentalists, and the investigations of mathematical theorists, have almost rendered the researches of modern philosophers hopeless of acquiring the truth from theory alone.

We shall merely notice a few of the many observations which have been made to determine this problem.

* A mixture of twenty pounds of alcohol and sulphuric acid gave only 25 grains of this dried powder.

The

The following are the velocities of sound in a second, according to the authorities of the names subjoined, expressed in English feet:

Roberval	560	
Gassendus	1473	
Mersennus	1474	
Duhamel	1338	
Newton	979	from theory.
Derham	1142	
Cassini,&c	1106.	

We shall not stop to examine the source of these discordant results, but merely observe, that the remarkable disagreement exhibited by these philosophers, in all probability, arises either from their not using proper pendulums vibrating seconds or aliquot parts of a second, but strings suspended with a plummet oscillating :—or, the distances of the stations from whence the sound was propagated, and observed, were either too near each other, or not exactly ascertained, so as to render any degree of precision to be expected from their inquiries.

The experiments of Dr. Derham are perhaps the most to be relied upon of any that have been made in this country; but the spaces through which the sound was propagated were not rigorously verified. This objection does not apply to the labours of Cassini, Maraldi, &c. who made these observations in France about the year 1738. They observed the velocity of sound under a variety of circumstances,—during a diversity of winds, in calms, and different changes of weather,—with nearly the same results: besides, the distance of one station from another had been previously determined, with accuracy, in measuring the terrestrial meridian through France. The mean velocity of sound, as deduced from their experiments, is 1038 Paris feet in a second; and if the length of the French foot be to the length of the English foot as 1·06575 to 1, as stated by General Roy, the distance will be equal to 1106 English feet; and it is highly probable, that this determination is more to be relied upon than any other experiments which have hitherto been made.

It may not be improper here to mention the excellent opportunity afforded us in this country, of settling this point in the most satisfactory manner. I allude to the many favourable situations which present themselves, whose distances from each other have been most accurately measured during the grand Trigonometrical Survey of England by Lieut.-col. Mudge, &c.

The theoretical principles of the propagation of sound, advanced by Sir Isaac Newton in the 47th proposition of his Mathematical Principles of Natural Philosophy, are simple, elegant, and appear consonant to the visible operations of nature, so far

as

as he proceeded upon the true principles of philosophy: his subsequent surmises are not worthy of notice, except as coming from such an exalted character.

This great man has demonstrated in the fore-mentioned proposition, that the number of pulses propagated is the same with the number of vibrations of the tremulous body; that they are not multiplied in their progress; and that as soon as the pulses cease to be propagated from the tremulous body, it will return to a state of rest. Again, in the 48th proposition of the same book, Newton has clearly solved an important question, in favour of the theory we have advanced; viz. that the velocities of the pulses are in a ratio compounded of the subduplicate ratio of the density of the medium inversely, and the subduplicate ratio of the elastic force directly.

If the atmosphere were of a uniform density at different distances from the earth's surface, the velocity of sound as deduced from the law of pendulous bodies would be exact; but, as the air is a medium that decreases in density as the distance from the earth increases, it appears to me to require a different view of the subject.

When the height of the mercury in the barometer and the temperature of the atmosphere are obtained, we can easily find the altitude of a column of air that would reach to the top of the atmosphere; and if it were of an equal density and elasticity in all its parts, the velocity of sound as recorded by Newton would be rightly stated. But it is otherwise; the atmosphere is not equally dense at every elevation, which the proposition of Newton requires. The question is then, What force is necessary to overcome the elastic power of that portion of the air which exists above the assumed height of the atmosphere, in order to compress the whole mass of the air within that limit?

Before we proceed in the investigation of the proposed subject, several preliminary inquiries are requisite. It appears from experiments carefully made by Mr. Tralles*, that the density of dry air at 32° of Fahrenheit's thermometer, and under a pressure of 29·92 inches, is equal to ·0012991 8, the specific gravity of water being taken as unity, and under the same circumstances; and to ·0012770 when the same thermometer is at 39°·83, which point Mr. Tralles has ascertained to be the maximum density of water, the barometer remaining as before. The specific gravity of mercury is 13·59925 at the former standard, and 13·59655 at the latter. Now by taking a mean between the experiments made by Gay-Lussac and those of Dalton on the expansion of dry air, it appears that for every additional degree of Fahrenheit's

* Nicholson's Journal, vol. xxv. p. 79.

thermometer

thermometer a corresponding dilatation of the air equal to ·002078 is produced.

According to the researches of Sir George Shuckburgh, the mean annual range of the barometer at the surface of the sea is 30·04 inches, and this elevation is constant in every degree of latitude. In calculating the temperature, we have followed the formula invented by that great philosopher Tobias Mayer of Göttingen. From these data we find the mean annual temperature at the surface of the sea in latitude 45° equal to 57°·5 of Fahrenheit; and the mean density of the air ·0012353; and by making use of the numbers ascertained by Mr. Tralles for the density of mercury, we find it in the same latitude and temperature 13·59043. Then to find the elevation of a column of air of equal weight, we have

$$\frac{13{\cdot}59043}{{\cdot}0012353} \times \frac{30{\cdot}04}{12} = 27540{\cdot}98 \text{ feet, or } 4590{\cdot}163 \text{ fathoms}$$

for the height of the atmosphere.

Now to find the pressure of the air, or, in other terms, at what elevation the mercury in the barometer would stand, if taken to the height of 4590·163 fathoms, is an important part of what we require.

In order to approach as near to accuracy as possible, it will be requisite to determine the precise decrease of temperature for every increase of altitude in the atmosphere. Saussure has found a decrease of one degree of Fahrenheit for every 289 feet. Daubuisson* found, under the same circumstances, an elevation of 319 feet corresponding to 1° upon the same scale. The best observations for deciding this question, we think, are those obtained from the journals of the Monks residing in the venerable priory situated upon Mount St. Gothard in Switzerland. The mean annual height of the barometer kept in this place is 21·77 inches, and of Fahrenheit's thermometer 29°·8. Now the latitude of the place is about 46° 30′ North; and the mean annual temperature of this latitude at the level of the sea, as appears from Mayer's formula before mentioned, is 56°·1. Calculating the height of the priory from these data, by De Luc's method for ascertaining the altitudes of mountains by the barometer, we find it equal to 8557 feet.

$$\text{Then } 56^\circ{\cdot}1 - 29^\circ{\cdot}8 = 26^\circ{\cdot}3, \text{ and } \frac{8557}{26{\cdot}3} = 325 \text{ feet}$$

of elevation, corresponding to a depression of 1° upon Fahrenheit's scale.

From these deductions we shall be able to find the height of the mercury in the barometer, at any determinate altitude, by applying the following calculation to that purpose.

* Nicholson's Journal, vol. xviii. p. 150.

27540·98

$\frac{27540 \cdot 98}{325} = 85°$ depression of the thermometer below $57° \cdot 5$ the station at the level of the sea; and to make the first correction for the expansion of the mercury, we have

$\frac{85° \times 30 \cdot 04}{9600} = \cdot 266$ of an inch: this quantity deducted from 30·04 leaves 29·774 inches, the correction for the barometer.

Then the logarithm of 29·774 is	1·4738372
Altitude of the air in fathoms ..	·4590163
Difference ..	1·0148209

The correction for the expansion of the air, reduced to the standard temperature 31°, as is done in calculating heights by the barometer, we find thus:

Temperature at the surface .. 57·5

27541 feet = a depression of .. 85°, or $27° \cdot 5$ below zero.

and $\frac{-27° \cdot 5 + 57 \cdot 5}{2} = +15°$ the mean heat; and $15° - 31° = 16°$: hence we have this proportion:

480 : 16° :: 4590·163 : 153·005 the correction for the condensation of the air.

Log.	1·0148209
Correction ..	·0153005
Inches 9·99	·9995204

the logarithm of the height of the barometer at the elevation of 4590 fathoms. From hence it appears demonstrable, that in order to compress the whole circumambient air within a circle whose semidiameter is 27541 feet greater than that of the earth, it would require an additional force equal to a pressure of 9·99 inches of the barometer, or nearly one-third of the whole weight of the atmosphere. Therefore, to resolve the proposed problem correctly, we must augment the velocity of sound, as deduced from the Newtonian theory, in the subduplicate ratio of the density of air, under a pressure of 30·04 inches, and 20·05 respectively. The density of air made use of in the preceding part of this memoir is ·0012353, the square root of which is ·03514. The air under a pressure of 20·05 inches requires a correction for temperature; we shall take its variation from the two extremes at 42°.5.

Then $\frac{\cdot 0012353 \times 20 \cdot 05}{30 \cdot 04} = \cdot 000824493 \times 42° \cdot 5 \times \cdot 002078 =$ $\cdot 000072815$, and $\cdot 000824493 + \cdot 000072815 = \cdot 000897308$ whose root is ·02996.

To find in what space of time a pendulum whose length is 27541 feet will make one vibration; we shall assume the measure of the one which oscillates seconds, in latitude 45°, as equal to 39·117 inches, or 3·25975 feet; and calculating from the established

blished law of pendulous bodies, we find the time in which it will make one vibration 91·766 seconds. But as the pendulum performs its going and return in the same time that an undulation moves forward over a space which expresses the velocity of sound in a second; therefore $91{\cdot}766 \times 2 = 183{\cdot}532$ seconds; and the space passed over in this time is the circumference of a circle whose radius is 27541 feet.

Therefore $\frac{27541 \times 2 \times 3{\cdot}141593}{183{\cdot}532} = 942{\cdot}9$ feet, the velocity of sound in a second, according to Sir I. Newton's method of calculation.

To account then for the wide difference that exists between the numbers as deduced from this theory, and actual experiments; Newton makes surmises very much beneath his usual penetration: he first supposes the crassitude of the solid particles of the air, and the intervals between those particles, to be in proportion to their diameters as 1 to 8 or 9, and from this source he augments the velocity 109 feet. Still the velocity is deficient; and to account for it, he next takes in the vapours floating in the atmosphere, as being of a different tone from the air, and conjectures their proportion to be to that of the air as 21 to 20; and these vapours are calculated to accelerate the progress of sound in that ratio; thus increasing the velocity to 1142 in a second, or until the velocity from theory agrees with that obtained from experiment. That these conjectures are not even plausible, the most ardent admirers of Newton must admit. In the first place, the proportions of the diameters of the particles of air to the intervals between them are merely imaginary, and the quantity of vapours calculated as existing in the atmosphere is absolutely erroneous. If Newton failed ultimately, in this problem, to anticipate the law of nature, the superstructure is raised upon an unerring base that can never fail. He had done enough for glory; and the specks upon his fame are, like those upon the luminary of day, invisible to the unassisted eye. But while we admire the greatness of the man, let us not tamely follow his splendid path; the perfection of philosophy requires the unbiassed assistance of the highest flights of human intellect, conducted by reason, and completed by experiment and reflection.

The theory of La Place, though highly ingenious, is nevertheless very improbable. That a momentary evolution of caloric should take place during every vibration in the aërial medium, we cannot imagine. But allowing it really to be the case, though the evolution of caloric might be separated with such rapidity as to escape our means to detect it from a single tremor of the air; yet I say that a sonorous body, kept in a continual state of vibration, must finally render the separation of caloric evident to the

the sense. Independent of this consideration, we might inquire, how much the temperature of the particles of air must be raised, to cause an increase in the velocity of sound, as obtained from theory, amounting to nearly one-fifth of the whole?

What we assert constitutes the real error in Newton's calculation is this, that he omits nearly one-third of the elastic power of the air, by supposing the whole to be compressed within an assumed limit; and that, although the velocity of sound, as estimated from the altitude alone, is correct, yet it is evident to us, that to compress the whole of the atmosphere within the distance of 27541 feet from the earth's surface, an additional force, equal to nearly one-third of the incumbent weight of the atmosphere, is necessary: therefore the velocity of sound at the surface of the globe, as deduced from Newton's theory, must be increased in the subduplicate ratio of 30·04 to 20·05; and we have before ascertained these two numbers to be 3514 and 2996: therefore, $\frac{942{\cdot}9 \times 3514}{2996} = 1105{\cdot}9$ feet, the true velocity of sound in a second, agreeing with the observations of Cassini exactly.

By this method we have calculated the velocity of sound under the equator, and at the poles; and find it greater by about 53 feet at the former than at the latter place; and this difference will be observed between the fervid air of a summer's day and the chilling atmosphere of December's reign; and the velocity of sound in the latitude of London will be about 1102 feet—estimating the increase and decrease to correspond with the variation of the latitude at the rate of one-sixth of a foot for every degree.

An interesting paper was published by Modeste Perolette*, in which he has made it appear extremely probable that the action of the solar rays materially affects the propagation of sound. From his ingenious experiments I should infer that the velocity of sound during the day was greater by about $\frac{1}{50}$ of the whole than in the night, or 22 feet in a second. Every philosophical observer must have observed the striking difference between the notes of a sounding body at midnight and noon; also the very different tone of the globe itself between a dry surface, and when the ground is covered with snow.

I am, &c.

RICHARD WINTER.

To Messrs. Nicholson and Tilloch.

* Nicholson's Journal, vol. xxv. p. 28.

XLII. *On*

XLII. *On the supposed Evolution of Heat from Vegetables.*

Leskeard, March 19, 1814.

Sirs,—In your Magazine for February, Mr. Forster in a note on January 30th, in his meteorological journal, attributes the thawing of the snow around trees and shrubs to the "heat transmitted from vegetable bodies." Having observed the same appearance, I at first also attributed it to the same cause; but from further observations I soon found reason to reject such an explanation of the phænomenon, because the snow, which lies around any substance whatever is affected in a similar way. Stones and rocks, and large or small pieces of wood inserted in or lying on the ground amidst snow, present the same appearance precisely, as trees, shrubs, or any other living vegetables. Since then we cannot attribute the disappearance of the snow in the latter instance to heat, it strikes me that it may be accounted for in another way. During the late snowy season, I observed that the thaw uniformly commenced and proceeded only on the surfaces of the snow; even that next the earth being considerably affected. The surface adjacent to a rock or other inanimate substance thawed slowly for a short time, till a free passage for the air was formed; afterwards melting as rapidly as the upper surface, and receding gradually from the rock as a centre.

Thus, whatever the substance may be around which the snow has fallen,—when a thaw commences, all the surfaces of the snow absorb caloric in the same proportion, or nearly so; and consequently an uniform retreat of the whole depth of snow will take place, and a bare piece of ground will be seen around the substance, of whatever kind it may be, in the form of a rude circle.

I have not made the above observations as an argument against the supposition that vegetables give out heat, but merely to show that Mr. Forster's explanation of the phænomenon which has occurred to both of us, does not in all instances hold good. This he will have an opportunity of observing, in all probability, during the next winter. It would be interesting to try a few experiments in order to elucidate this matter; and also to determine, by the thermometer, if possible, whether or not heat is really evolved during the circulation of the vegetable fluids. Is there any sensible heat in cold-blooded *animals?* If not, it seems improbable that *vegetables* should afford any.

Yours, &c.

Cornubiensis.

To the Editors of the Phil. Mag.

XLIII. *Process*

XLIII. *Process for obtaining Iode.* By Mr. ALEXANDER GARDEN.

Old Compton Street, Soho, March 21, 1814.

SIRS,—THE following method, which I have lately employed for procuring iode, I take the liberty of communicating for insertion in the Philosophical Magazine, (should you deem it of sufficient importance,) and which, in addition to the information already therein contained on the same subject may not be altogether unacceptable to some of your chemical readers. It is as follows:

1. To a concentrated watery solution of kelp (or the waste ley of the soap-maker where kelp has been employed) from which the greater part of the crystallizable salts have been separated by the usual processes of evaporation and crystallization, add red oxide of lead in the proportion of one ounce to about a pint of the former; boil the mixture over a slow fire to dryness, and increase the heat towards the end of the solution so as to carbonize any animal or vegetable matter which the substance may contain. This last may most conveniently be performed in an iron ladle.

2. Digest the dried mass in a quantity of cold water sufficient to extract the greatest portion of the soluble matter, filter, and evaporate the clear liquid to the consistence of a syrup.

3. Introduce the liquid obtained in the last process, together with the saline matter which may have separated during the evaporation, into a glass mattrass, and pour thereon about twice its bulk of alcohol; digest with a gentle heat for a few minutes and then suffer the vessel with its contents to grow cold.

4. Decant the clear alcoholic solution into a tubulated retort, adapt a receiver, and by means of a gentle and gradually applied heat distil off the alcohol. The saline matter which remains in the retort is to be washed out and evaporated to dryness in a capsule of Wedgwood's ware.

5. Introduce the dry salt thus obtained into a mattrass with a short neck, to which a glass tube about six or eight inches long, and sufficiently large to fit over the external diameter of the neck, has been luted, and pour over the salt by means of a long-necked funnel twice its weight of strong sulphuric acid. Heat the mixture gradually by a spirit or Argand's lamp, and the iode will immediately begin to rise in the form of a dense and beautiful violet-coloured gas, which will be condensed in the upper part of the vessel in black shining crystals; when the gas ceases to be disengaged the mattrass should be allowed to cool: the iode may then be washed out with water, dried on white filtering paper at a temperature not exceeding 100° of Fahrenheit, and inclosed in a bottle accurately fitted with a glass stopper.

The chief advantage of the preceding processes is to separate

 pretty

pretty completely all the foreign salts contained in the kelp, and to obtain, in a nearly insulated state, the saline compound, from which alone iode is to be obtained.

The mass remaining in the mattrass from which the iode has been separated, consists of sulphate of potash with a small proportion of sulphate of soda; hence it would appear probable that in kelp an iode may exist in combination with potash or soda, or both, as iodine of potash or of soda. When iode is added to a solution of subcarbonate of potash a combination takes place, and the mixture assumes a dark-brown colour. If a gentle heat be applied, this colour dissappears and the solution remains of a pale straw-yellow colour, a boiling heat does not separate iode from this combination, but if the solution be evaporated to dryness, the addition of sulphuric acid to the dry mass separates iode under its characteristic form.

I am, sirs, yours, &c.

ALEXANDER GARDEN.

To Messrs. Nicholson and Tilloch.

XLIV. *Correction of a typographical Error which occurs in the Statement of Mr.* ACCUM'S *Process of preparing Iode, published in the last Number of the Philosophical Magazine; with additional Remarks on the Method of obtaining Iode. Communicated by Mr.* ACCUM, *Operative Chemist, &c.*

Compton Street, Soho, March 1814.

SIRS,—I WILL thank you to notice in the Philosophical Magazine for next month the following typographical mistake, which occurs in some copies of that work, published last month; namely, for the words *oxide of lime*, page 146, line 6, read *oxide of lead*.

Having been applied to from different quarters concerning this mistake, you will perhaps do me the favour to reprint by way of answering my correspondents*, the paragraph again in which the error occurs. This paragraph should read thus:

"ANOTHER PROCESS OF OBTAINING IODE.

"Iode is to be found in abundance in the waste or spent lee of those soap manufacturers who employ kelp in the preparation of soap. To obtain the iode from the waste lee, let it be boiled for a few minutes with quicklime; strain the fluid and mingle it with sulphuric acid in excess†. This being done, evaporate the

* Mr. Accum embraces this opportunity of returning thanks to those correspondents who have honoured him with their inquiries concerning the above error of the press, &c. but reminds them that it is usual on such occasions, to pay postage.

† The boiling of the lee with quicklime is not essential, but it is necessary that the sulphuric acid should be added greatly in excess.

liquor

liquor to a syrupy consistence, and then distil, or heat it, in a flask, or retort with red oxide of lead and sulphuric acid. The iode will thus be obtained in abundance; and this in fact, constitutes the cheapest process of obtaining it."

If the product to be distilled with manganese has not been freed sufficiently from the muriates in which the soap lee abounds, there then is a copious production of chlorine, together with a yellow fluid, and then the quantity of iode becomes considerably diminished. This loss may be guarded against, by adding filings of zinc to the mixture, previously to submiting it to distillation. In fact, the addition of zinc filings effects the expulsion of an additional portion of iode from the mass, after the oxide of lead or manganese has ceased to act.

For the knowledge of this fact, I am indebted to the chemical professor of the Royal Institution.

It is obvious that instead of waste lee, the so called black ash, dissolved in water, may be successfully employed.

I remain, yours, &c.

FREDRICK ACCUM.

Messrs. Nicholson and Tilloch.

XLV. *On Preparing Iodine.* *By Mr.* JAMES FISHER.

Walbrook, March 1814.

SIRS,—MR. ACCUM having given to the public through the medium of your Magazine, some processes for preparing iodine, I have repeated them with success; and I beg, through the same channel, to point out another method of preparing iodine not mentioned by that chemist, but which has been found to answer well. The operation consists in concentrating the waste lees of the soap manufacturer, by boiling, and then mixing it with spirit of wine, or alcohol; the salts of the lee become precipitated, and fall down to the bottom, but the iodine remains in the spirit of wine. If the spirit now be distilled off, and the residue be then heated with a little sulphuric acid, and manganese, or red lead, the iodine sublimates in the neck of the retort. About one drachm of iodine was thus procured in the presence of the members of the new Chemical Society of London, from ten wine gallons of soap lees, and this substance is therefore unquestionably, as Mr. Accum observes, the best substance for obtaining iodine.

I am, sirs,

Your most obedient servant,

JAMES FISHER.

To Messrs. Nicholson and Tilloch.

XLVI. *On the Capacity for Heat, or Calorific Power of various Liquids.* *By* Benjamin Count Rumford*.

This subject is of rather an obscure nature, and it has been so little examined, that it will be useful to begin by elucidating it as well as I can.

Let us suppose two cylindrical vessels, with very thick sides, made of lead or any other metal, and perfectly equal in size, each being capable of containing a pint.

These two vessels being at the freezing point, we shall pour into the one a pound of water at the temperature of 96 F ($= 28\frac{1}{2}$ R) being that of the blood, and into the other a pound of olive oil at the same temperature.

Each of these liquids will heat the cold vessel in which it is placed, the vessel in its turn will cool the liquid, and both the liquid and the vessel will latterly be of the same temperature.

If water and oil of olives had the same calorific power, a pound of water at the temperature of 96° would heat its cold vessel precisely as much and not more than a pound of oil would heat its vessel, the two vessels being of the same weight, and at the same temperature at the commencement of the experiment.

But experience shews that water heats its vessel much more than oil does: consequently the calorific power of water is greater than the calorific power of oil of olives, when the *quantities* of these two liquids are estimated by their weight; and if we designate the calorific power of water by 1. the calorific power of oil of olives will be expressed by a fraction under 1.

The power with which any given body, solid or liquid, being at a given temperature, resists the calorific or frigorific action of bodies warmer or colder than itself, is in proportion to its calorific power; and the greater is this power, the longer it resists these actions of surrounding bodies.

If, under equal surfaces, a pound of water and a pound of oil of olives, both at the same temperature (96 F) are placed at the same time in a place where the temperature is lower (that of freezing for instance) the oil of olives will be cooled much more rapidly than the water.

If it be in a *warm* place that the two liquids are exposed, the oil of olives will still have its temperature most rapidly changed: it will be more heated than the water.

In two cylindrical glass vessels of equal size and very thin, place equal quantities of water and at the same temperature (96 F.)

* This paper was read before the French Institute as a supplement to the Count's Inquiry into the Heat developed by Combustion. Vide Phil. Mag. vol. xlii. p. 296, and vol. xliii. p. 64.

A piece

A piece of lead weighing a pound, and a piece of copper of the same weight, having been cooled in a mixture of pounded ice and water, remove them from this cold mixture and plunge each of them suddenly into one of the vessels of water.

The two masses of water will be cooled: but that which contains the copper most, for the calorific power of copper is greater than the calorific power of lead.

We may also say that the *frigorific power* of copper is greater than the *frigorific power* of lead, and in the case in question, the expression perhaps will be most suitable.

It is always the same power: it is that by means of which any body resists the action of surrounding bodies, and which tends to change its temperature either by increase or diminution.

Much obscurity has been introduced into the science by vague ideas, being attached to the words *hot* and *cold:* but it will not suit my purpose to enlarge upon this subject at present. I have already delivered my opinion in a former paper.

The little heat which I discovered in the condensation of alcohol, having induced me to think that the specific heat of this liquid had not been accurately determined, and wishing to know it precisely, in order to enable me to finish the calculations which were necesssary for elucidating the results of some of my experiments, I constructed a small and very simple apparatus, by the help of which I could easily, and as I presume accurately, determine it.

This apparatus consists of a small bottle of a particular form constructed of thin leaves of red copper, intended to contain the liquid which is to be the subject of the experiment; and a small cylindrical vase, also constructed of thin pieces of red copper in which I place water at a certain temperature. Into this water I plunge the bottle of copper containing the liquid which is the subject of the experiment: this liquid being of a different temperature from that of the water in the outer vase.

As the capacity of the vase for heat, as well as that of the bottle are known, I determine by a very simple calculation, the capacity for heat of the liquid contained in the bottle. This calculation which is well known, is founded in the changes which take place in the temperature of liquids, in the vase and in the bottle, by taking an uniform temperature, when the bottle is immersed in the water contained in the vessel.

In order that this equality of temperature may be speedily brought about, the form of the bottle is such that it has a very great surface relative to its small capacity, and in order to manage it without touching it, its neck which is small is closed by a long cork, which serves as a handle.

In order to diminish as much as possible the effect of the atmosphere,

mosphere, and of surrounding bodies upon the apparatus, while the experiment is going on, the quantity of water in the vessel is regulated so as to keep the bottle wholly submerged in the liquid, and even the upper end of the neck covered, when the bottle is immersed. The vessel which contains this water is placed and suspended by a ring of cork in another vessel larger and higher, and the interval between the two is filled with eider down.

The form of the bottle is such that its horizontal section presents the figure of a rectangular cross. Some idea may be conceived of its form and dimensions, if we suppose a square piece of stick, each facet of which is four lines broad by four inches three lines in length, upon the four faces of which we have fixed four sticks of the same length (*i.e.* 4 inches 3 lines) but each of them being four lines thick by eight broad.

The four sticks last described will exhibit the figure of the bottle: for the square piece of stick will be concealed by them from our view.

The neck of the bottle is in the prolongation of its axis: it is four lines diameter by four high: it ought to be circular: the cork should be an inch long, and the bottle weigh 76·07 grammes without its cork.

The cylindrical vase which contains the water is two inches diameter, and four inches nine lines high, and it weighs 74·65 grammes.

The exterior vessel in which the latter is suspended by the cork ring, is five inches three lines high and three inches diameter, so that the sides and bottom are every where separated by an interval of six lines: this interval is filled with eider down as already mentioned.

To prevent the water from touching the eider down, the cork ring is covered with a thin coating of mastic.

In order to ascertain the temperature of the bottle, and of the liquid which it contains, without being obliged to plunge a thermometer into the bottle, which would in this case be inconvenient, I employed a very simple method.

I placed a large bucket filled with water in a room with a northern aspect: I allowed it to assume the temperature of the room, taking care to shut the door and windows day and night. I placed the small bottle on a stand in this bucket, keeping the upper part of the cork only out of the water. As the bottle is small and has a large surface, it speedily acquires the temperature of the bucket of water, but in order to be well convinced that the bottle and the liquid which it contains have acquired the temperature in question, I leave the bottle a considerable time in the bucket, frequently half an hour and sometimes more.

In

In giving a detailed account of an experiment made with this apparatus, I shall have an opportunity of giving clear and precise ideas of the different parts of my apparatus, and of the particular objects which they are intended to attain.

Having found by various preliminary experiments made with water that the capacity for heat of the cylindrical vessel with that of the thermometer employed to determine the temperature of the water which it contained, was equal to that of 24·3 grammes of water, and that the specific heat of the bottle of copper was equal to that of 8·36 grammes of water. I made the following experiment with purified linseed oil.

I put into the cylindrical vessel 180 grammes of water: the temperature of the room was $59\frac{1}{2}°$ F. I filled the copper bottle with the above oil, and corked it. I cooled it in a bucket of water at the temperature of $44\frac{1}{4}°$ F. The oil in the bottle weighed 82·55 grammes.

The bottle having had time to acquire the temperature of $44\frac{1}{4}°$ F. was withdrawn from the bucket, and placed in a cylindrical vessel of tinned iron, of about four inches diameter, and six high, filled to the height of four inches and a half with water, at the temperature of $44\frac{1}{4}°$ F.

The bottle being submersed in this vessel of cold water, was carried into the room where I had placed the small vessel of copper belonging to the apparatus: it was then taken out of the cold water and plunged into the water contained in the small cylindrical vessel of copper which contained 180 grammes of water, at the temperature of $59\frac{1}{2}°$ F.

A thermometer having a cylindrical reservoir four inches long, which was placed in this vessel beside the copper bottle, soon fell, and in three or four minutes it marked $56\frac{1}{2}°$ of F. where it remained a long time stationary, and afterwards began to ascend slowly.

The capacities for heat of the warm bodies which were cooled in this experiment, were equal to that of 204·3 grammes of water; viz.

That of the water employed	180	grammes.
That of the vases and thermometer	24·3	
Total	204·3	

The capacity for heat of the bottle containing the oil was

equal to that of	8·36	grammes of water.
And to this we must add the cold water adhering to the bottle, when it came out of the cold water, and was plunged into the water contained in the copper vessel, I found by a particular experiment that this quantity of water was	1·04	
Total	9·40	

Now

Now as the temperature of the warm water in the cylindrical vase of copper was that of $59\frac{1}{2}$ before the mixture, and $26\frac{1}{2}$ after the communication of the heat had been obtained, it is evident that this water was cooled $2\frac{3}{4}°$. But if we multiply the number of grammes of water which the specific heat of this water represents, and that of the vessel $= 204·3$ grammes by the number of degrees which it has been cooled ($2\frac{3}{4}°$) we shall have a product which will express the number of grammes of water, which would have been cooled one degree of F. by a loss of heat equal to that which the vessel and its contents supported in this experiment. It is $204·3 \times 2·75 \times 561·84$ grammes.

We shall now see what part of this heat was communicated to the bottle, and to the small portion of cold water attached to it, and what part to the oil contained in the bottle.

As the temperature of the bottle and its contents was $44\frac{1}{4}°$ F. before the mixture, and $65\frac{1}{2}°$ afterwards, it is evident that the bottle had acquired $12\frac{1}{4}°$ of heat: consequently if we multiply 9·4 (the number which expresses the sum of the capacities for heat of the bottle, and of the cold water adhering to it) by $12\frac{1}{4}°$, we shall have a product which will express the number of grammes of water, which would have been heated one degree by the heat communicated during the experiment to the bottle, and to the small portion of water which adhered to it.

It is $9·4 \times 12·25 = 115·14$ grammes.

If from the heat lost by the vessel and the warm water which we have found equal to that which is necessary for raising the temperature of 561·86 grammes of water, one degree of F. .... 561·83 grammes.

We take the quantities which the bottle and the water adhering to the bottle have received 115·15

We shall have .... 446·69 grammes of water heated one degree, expressing the quantity of heat employed for raising to $12\frac{1}{4}°$ F. the temperature of the 82·55 grammes of linseed oil which were put into the bottle.

On dividing this number (446·69) by $12\frac{1}{4}$, we shall see how many grammes of water would have been heated one degree, by the quantity of heat in question.

It is therefore $\frac{446·69}{12·25} = 36·464$ grammes of water.

By the results of this calculation we find that the same quantity of heat which is necessary to raise the temperature of 36·464 grammes of water 12 degrees and a half of Fahrenheit's thermometer, is sufficient to raise the temperature of 82·55 grammes of oil the same number of degrees.

Consequently

Consequently the capacity of water for heat is greater than that of oil of linseed in the proportion of 82,55 to 36,464 : and if we express the capacity of the water by *unity* as is usually done, the capacity of the above oil ought to be expressed by the fraction 0·44172.

These details must no doubt appear superfluous to those who are versed in the higher branches of knowledge, and who are accustomed to express the most complete relations by algebraical signs: but it must be recollected, that the subject of which I treat is familiar to few, and that it is necessary to explain with rigorous accuracy the principles upon which the method employed is founded, as well as the manner of using the apparatus which I recommend.

On repeating twice the experiment made with pure linseed oil. I had as a result in one of these experiments a capacity for heat equal to .... 0·44411

And in the other equal to .... 0·47193

If to these two results we add that of the first experiment, equal to .. .. 0·44172

We shall have as a mean result .... 0·45192

The following are the results of some experiments made with other liquids. Olive oil furnished:

	Specific Heat of Olive Oil.
1st Experiment	0·45944
2d Experiment	0·43422
3d Experiment	0·42183
Mean result	0·43849

Three experiments made with naphtha gave the following result:

	Specific Heat of Naphtha.
1st Experiment	0·43408
2d Experiment	0·39234
3d Experiment	0·41905
Mean result	0·41519

Three experiments made with spirits of turpentine gave the following result:

	Specific Heat of Turpentine.
1st Experiment	0·29322
2d Experiment	0·37031
3d Experiment	0·34216
Mean result	0·33856

Two experiments with alcohol, of the specific gravity of 817624, gave the following results:

Specific

	Specific Heat of Alcohol.
1st Experiment	0·54924
2d Experiment	0·55063
Mean result	0·54993

Two experiments with spirit of wine of the specific gravity of 85324, gave the following results:

	Specific Heat of Spirit of Wine.
1st Experiment	0·57840
2d Experiment	0·58317
Mean result	0·58078

Two experiments with sulphuric ether of the specific gravity of 72880, gave as results:

	Specific Heat of Sulphuric Ether.
1st Experiment	0·53711
2d Experiment	0·54768
Mean result	0·54329

I was at first much surprised to find so great a capacity for heat in sulphuric ether, but my astonishment was diminished when I recollected that this liquid can unite with alcohol in all proportions, without exhibiting any symptoms of a chemical action: for this reason therefore, we ought to expect to find the same capacity for heat in both liquids.

XLVII. *Notices respecting New Books.*

Official Papers, relating to Operations performed by Order of the Directors of the Royal Hospital for Seamen at Greenwich, on several of the Pensioners belonging thereto, for the Purpose of ascertaining the general Efficacy of the new Modes of Treatment practised by Mr. Adams, *for the Cure of the various Species of Cataract and the Egyptian Ophthalmia. Published by Order of the Directors,* 1814. Printed by Winchester and Son.

We have already had occasion to notice the successful labours of Mr. Adams in that branch of medical science which regards the treatment of the diseased eye. We have now before us a most interesting publication occasioned by the same gentleman's professional exertions among the pensioners of Greenwich Hospital, and since printed by the express order of the directors, after a careful investigation of the merits of the practice which it recommends, the high sanction of official authority is now superinduced upon the well earned fame which Mr. Adams has already acquired.

In

In a short advertisement prefixed to the present publication, we are informed that the directors of Greenwich Hospital, having heard of the great improvements made by Mr. Adams, in the modes of operating on the different kinds of cataract, he was requested to examine the blind pensioners, with a view to ascertain what could be effected for their relief.

Mr. Adams accordingly selected twenty cases for operation, consisting of cataracts, closed pupils, and the Egyptian ophthalmia. For the temporary accommodation of these patients, a house in town was taken by the directors, and they were placed under the immediate care of Mr. Adams, who very handsomely disclaimed all expectation of remuneration for his services.

The Reports of the Medical Officers of the Institution having certified that the operations performed by Mr. Adams had been attended with the greatest success, a special meeting was called, at which the directors personally examined and interrogated the pensioners, who had been under treatment for diseased eyes, "as well those who had been under the care of former oculists, as the patients of Mr. Adams."

The result was highly honourable to the professional reputation of Mr. Adams and the directors considered it to be a duty which they owed to humanity, to give publicity under the authority of their names to the whole proceedings which had taken place.

In a communication dated May 25, 1813, from Mr. M'Laughlin, surgeon to Greenwich Hospital, that gentleman informs the directors "that the first set of patients sent to London, requiring thirteen eyes to be operated on, are all cured, with the exception of one man, David Hoar, a person of notoriously perverse character, who was attacked with fever, and sent back to the Hospital for the cure thereof; but even in this case there is every prospect of success, from a repetition of the operation.' Eleven other patients who were sent to replace those already cured, have all been operated on, and with the prospect of complete success (excepting two that had been previously couched by another oculist), five being already capable of seeing, and the others proceeding as favourably as could be expected.

"The superior success of Mr. Adams's new modes of practice, when compared with the operation performed on the pensioners for the last fifteen years, is very striking. On examining the latter it appears, that out of *twenty-four eyes* operated upon, several had been destroyed; in other instances the pupils had become obliterated, and *one only* had been benefited, and even that the success is incomplete.

"Among the men already cured, some of the cases are so remarkable as to merit particular notice.

"Edward

"Edward Turner had, during six months that he was a patient of the London Eye Infirmary, undergone *thirteen* operations; nevertheless, he obtained but very little benefit in one eye, and none in the other: Mr. Adams has cured both by *one operation on each.*

"Hartgill had been blind for near twenty years, and was considered by every oculist of eminence in London to labour under Gutta Serena. Mr. Adams has successfully operated on him, and he is now capable of reading the *smallest print.*

"Bray, aged seventy-nine years, is cured of cataract in both eyes, and was capable, in the space of a fortnight, of seeing the *minutest objects.*

"Douglas, aged thirty-two, with closed pupils, after having been above five years a patient of the oculist before referred to, without experiencing the least benefit, has had an artificial pupil formed, by which he is enabled so see the most minute objects with distinctness; his other eye has been since operated on, and promises to be equally successful.

"I cannot, in justice to Mr. Adams, conclude this Report, without expressing to you how greatly I am indebted to his liberality and disinterestedness in communicating to me much valuable information and instruction relating to diseases of the eye, which I trust will eventually be attended with considerable advantage to that class of objects, the afflicted blind, who constitute no small proportion of the invalids admitted into this noble asylum.

Subsequently Mr. M'Laughlin reported that in consequence of the absence of Mr. Adams from London, for a few months it was requisite to recal the pensioners to the Hospital—"and," this gentleman adds, "I have infinite pleasure in repeating that Mr. Adams's practice and operations upon them have been successful in a most extraordinary degree. Many are perfectly restored to sight; and all the others, where there was any reasonable prospect of a cure, though as yet necessarily only convalescent, are going on favourably; and should it be the pleasure of the governor and directors to review those that have undergone operations, I beg that they may be presented before them, for that purpose, at the next Board at the Royal Hospital."

A vote of thanks from the governor and directors succeeded these flattering testimonies; and, latterly, the whole medical officers of the Institution made the following communications to the Board, which we consider to be so interesting as to require no apology for giving at length.

"Royal Infirmary, Dec. 27, 1813.

"Sir,—We inclose herewith, for the information of the Directors, separate Reports of the results of the *new* and *old* operations

tions for the cure of cataract which have been practised upon the pensioners of the Royal Hospital, the great disparity in which cannot fail to make a strong impression on the minds of the honourable members of the Board.

"In order to bring them equally acquainted with the extent of the failures in extracting the cataract, as formerly practised, as well as with the great success of Mr. Adams's improved modes of curing that disease, we have given a detailed description of the result of each, with the present state of the eyes which have been submitted to the trial of the two systems.

"The proportion of the eyes totally destroyed by the operation of extraction amounts to one-half the number operated upon; to this the success of Mr. Adams, more particularly in the cases which had been considered incurable, as well as those previously operated upon without benefit, forms a very striking contrast, as it will be seen that his operations have failed but in one instance.

"To enable the Board fully to appreciate this success, we think it proper to point out, that even in the men whose vision is not at all, or but partially, benefited (with the exception of Ford), the operations were as perfectly executed as on those whose sight is completely restored. To the disease of the optic nerve, therefore, and not to the failure of the operation (as was the case where extraction had been formerly performed), is to be attributed the want of that perfectly successful issue which is so conspicuously displayed in the "*unexceptionable cases.*"

"This diseased state of the optic nerve in those patients was originally apprehended by Mr. Adams; and when, at their urgent solicitations, he was prevailed upon to perform the necessary operations, he stipulated, that, should the event confirm his unfavourable opinions, we should attest the circumstances under which they were undertaken.

"It is, however, very important to have ascertained, by actual experiment, as Mr. Adams has done on several of the pensioners, that the optic nerve, although so much diseased as to have deterred a former practitioner from operating, yet, by the removal of the cataracts, and subjecting the eyes to a particular plan of discipline, their functions have been sufficiently recovered to afford useful, and sometimes almost perfect, vision. An instance of the latter is shown in the case of Hartgill, blind for nearly twenty years, as supposed, by all the highest authorities in London, from gutta serena, for which disease he had been treated. Bray's and Wilkins's perfect restoration to sight are little less extraordinary, from the great age of the former, and the latter having had an artificial pupil formed after a complete obliteration of

of that aperture, by an unsuccessful operation of extracting the cataract, performed seven years since.

"These, together with the other cases included in the two last divisions of the Report No. 2, prove that a very large proportion of persons unhappily afflicted with blindness, and hitherto considered incurable, are now susceptible of relief from the new and improved operations, and the after-management of the eyes, practised by Mr. Adams.

"In addition to the gratifying contents of the second Report, we think it our duty to state, for the information of the Board, that Mr. Adams has discovered a mode of curing the Egyptian ophthalmia, which has been successfully practised upon several of the pensioners, some of whom had been blind for three or four years, and given up as incurable by the most eminent oculists then in London. The communication that this destructive and hitherto intractable disease admits of cure we conceive will be gladly received by the Board, and the promulgation by Mr. Adams of this important discovery be considered as a great *national desideratum.*

"By the adoption of his practice we are of opinion, from what we have seen of its effects, that a very large proportion of the seamen and soldiers, who have been discharged the service, blind of the ophthalmia, might be again rendered fit for duty, or be made useful members of society.

"We cannot conclude this letter without stating, in justice to Mr. Adams, that he has freely demonstrated his practice; and that he has, in the most liberal and unreserved manner, given us every information that we required relating to the treatment of diseases of the eyes.

We are, sir, your very humble servants,

R. Robertson, Physician.
B. M'Laughlin, Surgeon.
M. S. Kent, Apothecary.

John Dyer, Esq.

RESULT

RESULT of the OPERATION of EXTRACTION which had been performed on Pensioners blind of Cataract now in the Hospital, previous to the employment of Mr. ADAMS.

REPORT I.	Eyes destroyed	12
	Obliterated Pupils	4
	Gutta Serena and secondary Cataract	3
	Opaque Cornea, and other diseased changes of the Eye	4
	Successful	1
	Total number of Eyes upon which the operation of Extraction had been performed	24

RESULT of the NEW OPERATIONS performed on the Pensioners by Mr. ADAMS.

CASES considered unexceptionable.

RRPORT 2.	Age.	Description of Disease.	Number of Eyes operated upon	Result of the Operations.	State of Vision.
John Bray	80	Cataract	2	Perfect	Perfect, being able to see as well as ever with Cataract Spectacles.
Richard Collins	63	Do.	2	Do.	Perfect, do. do.
Robert Kinsley	70	Do.	2	Do.	Sight returning, when he died from Carbuncle.
Robert Handcock	47	Do.	1	Do.	Perfect, and gone on Out-Pension.
*Jonathan Stratton	75	Do.	1	Do.	Perfect.
Edward Hilback	40	Do.	1	Do.	Do.
Guy Overton	79	Cataract, with obliterated pupil	1	Cataract cured, and an Artificial Pupil formed	Do.
Silas Darby	66	Cataract	1	Perfect	Do.
William Russel	50	Cataract complicated, with obliterated Pupil, and a defect of the Optic Nerve	1	Cataract cured, and an Artificial Pupil formed	Vision nearly, but not quite perfect, owing to the diseased state of the Optic Nerve.
William Roberts	48	Cataract	1	Cataract cured, partial opacity of the Cornea remaining	Vision not perfect, but sufficient for the common purposes of life.
Thomas Ford	74	Cataract	1	Unsuccessful, but no deformity produced	

CASES

CASES considered incurable by former Oculists.

John Douglas	36	Cataracts with obliterated Pupils	2	Cataracts cured, and artificial Pupils formed	Perfect.
Frederick Hartgill	48	Cataracts complicated, with Gutta Serena	2	Perfect	Was enabled to read small print with great fluency, till attacked with violent inflammation of the eyes from cold. Now convalesc.
Thomas Dailey	46	Cataract and obliterated Pupil, with Gutta Serena	1	Cataract cured, aad an artificial Pupil formed	Vision useful, and can see small objects.
William Austin	31	Do. Do. Do.	1	Ditto.....................	Vision useful, and able to see objects at a considerable distance.
David Hall.	52	Cataract and obliterated Pupil..	1	Convalescent, when discharged from the Infirmary for incorrigible drunkenness	
William Thompson	49	Do. Do.	1	Cataract cured, but Pupil requires further enlargement..	Vision useful and improving, when he died of Consumption.
†Otter Grindall	40	Cataract and Gutta Serena......	1	Perfect	Vision useful, and continues to improve.
Thomas Whiteman	28	Cataracts (adherent) with a defect of the Optic Nerve	2	Do.	Vision very good, but not perfect.

CASES which had been operated upon unsuccessfully by former Oculists.

Edward Turner	46	OneCataract adherent, the other detached and floating. Had previously undergone 13 operats.	2	Perfect.....................	Perfect.
John Broadway....	51	Cataracts membranous (or secondary), and Gutta Serena..	2	Do.	Not improved, Optic Nerve being totally insensible.
John Maddin......	52	Do. do. with obliterated Pupil	1	Membranous Cataract removed and an artificial Pupil formed	Do.
John Wilkins......	52	Cataract secondary, with obliterated Pupil, from the failure of Extraction seven years since	1	The secondary Cataract removed and an artificial Pupil formed	Perfect, being able to see the minutest objects.
		Total number of Eyes upon which the new Operations have been performed	31		

RECAPITULATION.

Successful	29
Unsuccessful	1
Discharged for irregularity ..	1
Total....	31

R. ROBINSON, *Physician.* B. M'LAUGHLIN, *Surgeon.* M. S. KENT, *Apothecary.*

Dec. 27, 1813.

* Jonathan Stratton was attacked, three months after he was cured, with repeated violent inflammations of the Eye, which assumed an intermittent form, and has nearly destroyed the Vision.

† Otter Grindall is since dead of Apoplexy.

In

In a letter to the Governor and Directors explanatory of his practice, which follows the above interesting documents, Mr. Adams observes, "from the statements which have been made of the success of the practice of extraction, the public have been taught to believe that it possessed all the excellence of which any operation for the cure of cataract was susceptible. It became, therefore, highly necessary that such an experiment as the present should be instituted; and that, under the immediate superintendance of *impartial* and *disinterested* persons, whose testimony could not be doubted."

All personal allusions to other operators are thus handsomely disclaimed.

"And here I beg leave to repeat the observation I made at my first interview with your honourable Board, that it is the *operation*, and not the *operator*, which I deprecate. Were he to adopt my operations, or were I to follow his, the results of the two modes of practice would probably be nearly the same as they are now found to be; nor shall I hesitate to add my firm belief, that superior manual dexterity is not to be found in this kingdom, than is possessed by the operator whose efforts have proved so unavailing, in the many instances submitted to your consideration. It is, I conceive, the want of a personal experience of the superior effiacy of my practice, which prevents his adopting it with the same promptitude, as another oculist of long-established celebrity has done, since he saw me operate; who, before that period, was distinguished by his practice, as well as writings, as one of the warmest advocates of the operation of extraction.

"It may be proper to inform your honourable Board that I have not confined myself to any individual operation in the treatment of the pensioners blind of cataract intrusted to my care. My instruments and modes of operating have varied as the nature of the case required. Where the consistence of the cataract has admitted of an immediate and complete division, I have placed the separated portions in a situation which insured their absorption in five or six weeks. In these cases, the general success of the operation exceeds all credibility with those who have been in the habit of witnessing the results of other modes of practice. Of upwards of *eighty persons* born blind of cataracts, upon whom I have performed this operation, I have not lost an eye. In three instances alone, in which I was prevented from repeating the operation, it did not produce the anticipated benefit; and I should consider myself unfortunate were I at any time to be less successful in an equal number of persons who became blind from cataracts after birth, provided they admitted of being treated in the manner already described, and the health of the patients was

in a state favourable for the operation. Hence arises a very important question: To what period of life does this particular practice apply? To which I have a ready answer;—That I have *never failed* in being enabled to effect this necessary division in persons under *forty years* of age; very rarely in those between *forty* and *fifty*, and have frequently succeded in persons in the most advanced periods of life.

"Where the cataract is too hard and solid to admit of this immediate division, I do not attempt, as was my former practice, to effect its absorption by a frequent repetition of the operation; but I at once extract it. This, however, is accomplished by a process *totally different* from that I have felt it a duty to deprecate; a process which I must claim to be novel, and which happily attains the highly important desiderata which had been hitherto considered *unattainable*, while it obviates the many causes of failure which rendered the usual mode of extraction so generally unsuccessful. From the principle upon which it is founded, and the favourable results of its termination during the last two years that I have extensively practised it, I feel myself warranted in asserting that it possesses the utmost degree of excellence which it is *possible* for extraction to arrive at, and that its general success will prove nearly as great as the operation for the removal of the soft cataract. To deter other persons from claiming it as their invention, or anticipating me in its communication to the public (as was the case with my instruments and operation for the cure of the *soft cataract*, and my successful revival of an obsolete operation for *artificial pupil*), I have requested Mr. M'Laughlin to record on the Hospital books, the different stages of this operation, as he has seen me perform it on several of the pensioners.

"I trust that it will not be considered as irrelevant to the subject of the present communications to inform you, that there are different modes of effecting the cure of cataract by the *absorbent practice.* My friend and preceptor, the late Mr. Saunders, pursued a system different from that which I have so warmly supported in this letter. The operation which he preferred had been performed *thirteen times* during six months on one of the pensioners (Edward Turner) without a removal of the disease. On one of my private patients the same operation had been performed *seventeen* times prior to my having been consulted, *ten* times on one eye, and *seven* on the other, in the course of as many months, and with no better success. In both instances I perfected the cure by a single operation on each eye; so that, if these patients had originally been treated according to my mode of practice, *one,* or at most *two* operations, would have

have effected the complete removal of the cataracts in the space of five or six weeks. This difference in the two modes of operating, it is of great importance to myself distinctly to specify, otherwise, from its being generally known that I was the sole confidential pupil and assistant of the late Mr. Saunders in his operations for cataract, among those who are now pursuing the profession of an oculist, it might be considered by many, who have not seen my work on diseases of the eye, that I still, as in the commencement of my practice, follow his modes of operation, whereas I have long since found it necessary wholly to abandon them."

In the formal official report, with which the publication concludes, the directors state that having examined the pensioners "on whom operations had been performed by other oculists of undoubted character and eminence, it appeared that many of these pensioners had irrecoverably lost their sight, the eye being in several instances entirely sunk, and, except in one instance, none had experienced the desired relief."

The directors then proceed to pay the following just tribute to the merits of Mr. Adams:

"The pensioners who had been under the care of Mr. Adams were next examined and interrogated, and their respective cases were compared with the report enclosed in the above-mentioned letter from the physician, surgeon, and apothecary; and the board were much gratified by so many instances of the great success which had attended the operations of Mr. Adams. The effect of those operations appeared to be accurately in the report in question."

While we congratulate our readers on the manifest improvements thus recorded in the treatment of that most delicate of all human organs, the eye, it affords us additional satisfaction to learn that the directors of Greenwich Hospital have requested Mr. Adams to continue his praiseworthy exertions, having placed an additional number of pensioners under his care.

XLVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 24, and March 3. The Right Hon. President in the chair. A long paper by Dr. Herschell was read, detailing the result of many years observations on the sidereal and nebulous appearance of the heavens. The Doctor began by relating his observations on the relative magnitudes of the stars, considering those of the first magnitude to be equal to our sun; determined the magnitudes and changes in the appearance of a great number of fixed stars;

stars; gave a history of the alterations which he has noticed in the aspect of the sidereal heavens, during the last 30 years; and described those stars which have increased in magnitude, or brilliancy, have lost or acquired surrounding nebulæ, or have had wings, tails, or other peculiarities. He seems inclined to believe, from his observations, that new sidereal bodies are in a constant and progressive state of formation, that nebulous appearances gradually assume a globular character; that the heavens are not infinite, and that stars have a "compressing power." He considers the origin and progress of sidereal bodies to be nearly in the following order: first, vague and indistinct nebulæ, like the milky way; second, detached or clustered nebulæ, which consolidate into clusters of stars; thirdly, these stars becoming more definite appear with nebulous appendages in the different forms of wings, tails, &c.; and lastly, that all are finally concentrated into one clear, bright, and large star. Dr. H. concludes that the progressive discovery of nebulæ will be equal to the improvement of our telescopes, and that in proportion as we are possessed of more powerful space-penetrating instruments, will our knowledge of the sidereal heavens be extended. Many of his latter observations directed to ascertain the absorption or condensation of nebulæ were made on stars, which he had before described in his numerous papers in the Phil. Transact.; others were made on those whose places have been determined by foreign astronomers.

March 10. Mr. Seppins, one of the surveyors of the Navy, in a letter to the President, described his new system of ship-building; he observed that notwithstanding the rapid progress in all the arts and sciences, no improvement in naval architecture has taken place during many years. In order to make the simple but great improvement which he has introduced more intelligible, he began by describing the old structure of ships, of their keel and ribs or timbers placed at right angles, and the bottom and decks composed of parallel planks. According to the new construction, on which three ships have already been built, and four more are building, the timbers are crossed with diagonal girders at angles of 45, so that the whole frame is rendered much stiffer or more inflexible, and all parts of the structure made to bear their due portion of the pressure at the same time. The first advantage of this plan is the prevention of what is called *hogging*, or having the centre become convex on the upper, and concave on the lower side. Mr. Seppins fills up the space between the timbers with pieces of wood taken from old ships, made in the form of wedges, which are reversed, driven in tight, paid with tar, and made impervious to water, so that should an outer plank start, the vessel will be in no danger of sinking

sinking, as in the old system. This method not only adds greatly to the stiffness and strength of the vessel, but also prevents the timbers and flooring from becoming a prey to the rot occasioned by moisture and stagnant air. Mr. S. exposes the notion of ships being elastic, and contends that they are stronger and better in proportion as they are non-elastic, and capable of resisting pressure in whatever direction it may be applied. Considerable advantage he also considers must attend his plan, from the superior stiffness and strength of the decks, composed of frame-work with diagonal binders, so that the deck, instead of being a series of parallel boards, having very little connection with each other, and susceptible of being detached in any emergency, will present a continuous mass of timber, having its grain placed in all directions best adapted to make the greatest possible resistance to any external force. There are many other minor improvements in this new method, such as obviating the necessity of much iron work, so that no extra-weight is occasioned by the filling-up between the timbers; less ballast is required; much old ship-timber can be used with advantage; and lastly, in the construction of a 74 gun ship, 178 trees, of 50 feet each, are saved.

March 17. A paper by Dr. Alex. Crichton was read, containing an account of his experiments on the vitality of organized matter. The author having observed that organized bodies are influenced by laws very different from those of chemistry, that living matter overcomes affinity and gravity, and that whenever life ceases, the decomposition of organized bodies commences, seems thence to infer that there are two kinds of matter, or that organized matter still retains some latent vitality, notwithstanding its chemical decomposition. To ascertain this point, he made a variety of experiments on different vegetables, on dried barks, flowers, &c. using decoctions of vegetable matter exposed to the action of oxygen and other gases in glass tubes over mercury, and in all of them, except a decoction of liquorice root, he discovered traces of vitality or fructification in a few days. The leaves of flowers he always found yielded the greatest quantities of organized or vitalic matter.

March 24. Dr. Young, with that acuteness and facility of applying mathematical principles to practice for which he is distinguished, presented a paper to the Society, which was read, consisting of remarks on the principles and improvements in naval architecture proposed by Mr. Seppins.

IMPERIAL INSTITUTE OF FRANCE, FOR THE YEAR 1813.

Mathematical and Physical Sciences.

M. Delambre, the secretary to this branch of the Institute, commences his report by lamenting the death of M. Lagrange, and the consequent suspension of the publication of the *Méchanique Analytique*, the printing of the second volume of which had just commenced when the author died.

M. Laplace, the report proceeds, who last year favoured the scientific world with a complete Treatise on Probabilities, has this year applied his theory to one of the most abstruse questions in astronomy; viz. the origin of comets and the nature of their orbits.

Dr. Herschel's opinion on this subject is fresh in every one's recollection. Perceiving almost every where in the celestial regions a faintly luminous matter, interspersed with points, denser or more luminous, he conceived that in time the system of universal attraction might unite round these centres the nebulous matter with which they are surrounded; that in consequence of their mutual attraction two or more of these centres might acquire a movement, that this motion might carry them to the surface of the sphere over which the attractive energy of the sun exerts its influence; and that this motion, combined with the solar attraction, might convert these centres into as many new comets circulating around the sun, and obeying the same laws as the planets: that this may have been the origin of all the planets, and also of the sun and stars; for if we are constrained to admit the anterior existence of these immense bodies, we may with equal propriety assign the same date to the lesser bodies which circulate around them.

These new orbits will of course be circular, elliptic, parabolic, or hyperbolic.

In the case of the orbits being circular, they will be always invisible, unless, in defiance of probability, we suppose that their mass and inherent lights are sufficient to render them visible at so great a distance, for the nuclei or centres of Dr. Herschel are invisible by a common telescope.

If the orbits are elliptical or parabolic, the comets may come so near the sun that they will become visible in a portion of their orbits comprehended between the perihelion and the parometer, and even somewhat beyond it. This supposition accounts with tolerable plausibility for the phænomena hitherto exhibited by comets. Their larger axis must go beyond the sphere of the sun's energy, which must extend much further than the orbit of Uranus. Such elongated ellipses will evidently be confounded with

with parabolas, having the same summit. The revolutions of such comets will be so tedious that we can scarcely expect to see or know them again, after the many alterations which they may have undergone in that part of their orbit, out of the reach of all human view, and where so many causes may modify their elements.

The above, if adopted, would also account for the length and tenuity of their tails. The nebulous matter, condensed by attraction so as to form the comet, being dilated by the solar heat, would resume nearly its primitive tenuity, and might even evaporate and be lost in space. Having thus lost its tail and its nebulosity, the comet will be exactly similar to the four little planets. It may even by being dissipated, cease to exist, and astronomers might lose all their labour in calculating its elements. It would be sufficient for them to be in possession of the coarse approximation, which puts it in their power to satisfy the public curiosity, without much trouble, during the short time that the comet is visible from our earth.

Some comets, however, have exhibited peculiarities inconsistent with this hypothesis. Halley's, for instance, has the great axis of its orbit smaller than that of Uranus. The comet of 1770 had a great axis less than that of Jupiter. According to M. Laplace, these singularities may be produced by the planetary attractions or the resistance of the æthereal medium; but the planetary attractions ought to be very weak at the entrance of the comet into the sphere of the sun's energy; and Laplace himself has rendered the resistance of the aërial medium very problematic and inconsistent with the constancy of the great axis of the planetary orbits.

If an elongated elliptical orbit be confounded with a parabola, the difference is not greater between the parabola and hyperbola. The insufficiency of the parabola in some cases having been ascertained, it has been found necessary to have recourse to the ellipsis. How has it happened that the necessity for the hyperbola has never been felt? Of 117 comets whose elements we are acquainted with, two only are decidedly elliptical: the remainder are parabolic or elliptical. In fact there is nothing to prevent us from considering these orbits as hyperbolic, but differing infinitely little from the parabola, and in this case there would be no occasion for surprise at the nature of these orbits having escaped us. The elliptical orbit of the comet of 1759 was only ascertained by its various returns in the interval of 75 or 76 years, and but for these returns its orbit would have been regarded as parabolic. Hyperbolic comets never return; and hence we have no opportunity of rectifying any mistake: hence also we have no evidence that the hyperbolic orbits are more rare than the elliptical. They may be even much more numerous

 without

without any suspicions being entertained on the subject, but M. Laplace confines his observations to those only whose hyperbolic orbits may be recognised by actual observation. In fact we are not acquainted with one of the kind.

Why are hyperbolic orbits so rare? This is a question which cannot be completely answered; and all that we can do is to apply to it the calculus of probabilities. If, among several cases, all appearing equally probable, there be one which seldom or never occurs, we may be authorised to conclude that there exists a cause for its being so rare. The chances which give a parabola must be very few in comparison with the others. M. Laplace, in fact, has ascertained that we may safely predicate *that a nebulosity which penetrates within the sphere of the sun's activity, so as to be capable of being seen, will describe a very elongated ellipse or a hyperbola, which, from the magnitude of its axis, will sensibly coincide with a parabola in the part observed.* It follows, from the analysis of M. Laplace, that in the case most favourable to hyperbolas it is 56 to 1 that the hyperbola will not be perceptible. Thus we might almost exclude this curve, and it would be so much calculation saved. In fact, the hyperbola is never tried till both the parabola and ellipsis have failed.

[To be continued.]

XLIX. *Intelligence and Miscellaneous Articles.*

FOSSIL HUMAN SKELETON.

This singular fossil is now placed among the minerals in the British Museum; the mass of stone bearing the skeleton, is fixed nearly erect in one of the glass compartments; it is little more than four feet long, about two broad, and from four to nine inches thick. Although the head, neck, and feet are wanting, it is evident that the being to whom these bones belonged must have been of a stature rather less than men in general. The finger-bones of the left hand are situated so closely between those of the pelvis and thigh, being almost touching, that all the integuments must have been destroyed before these bones were enveloped with the calcareous matter. The block itself is a fine granular limestone, neither so compact as to appear uniformly crystallized, nor so porous as our common calcareous sandstones. There is some very similar limestone in the neighbourhood of Maidstone and West Malling, in Kent; the chief difference being that parts of the fragments of shells in this Guadaloupe stone are a bright red, while those in the Kentish stone are gray or yellowish. Its fracture presents an appearance between that of a calcareous

calcareous stone formed by simple deposition, and by imperfect crystallization; it is just such as might reasonably be expected to be formed in the vicinity of a volcano, where the solvent was considerably above the natural temperature of water, and much below that of metallic fusion.

SOUNDNESS OF PRECIOUS STONES.

It is of great importance to lapidaries, and purchasers of precious stones, to possess some means of ascertaining their soundness or freedom from flaws when in the rough state. Hitherto artists had no rule to direct their judgement in this respect, and have in consequence often sustained much loss. To remedy this defect, Dr. Brewster proposes the following method: Immerse the rough unwrought stone in Canada balsam, oil of sassafras, or any other fluid of nearly the same refractive density (as oil of anise seeds) and turn it round with the hand so that the rays of light may pass through it in every direction. By this means the slightest flaws or cracks may be instantly perceived in consequence of the changes which they produce on the transmitted light. If the stone be examined in water the flaws become more perceptible than when viewed in the air; and the distinctness with which they are seen increases as the refractive powers of the fluid approach that of the solid. Thus diamond jargon, spinelle ruby, &c. which exceed any fluid in refractive power, have their imperfections detected when immersed in oil of cassia or muriate of ammonia. Natural and artificial stones may be likewise discriminated by oil of cassia, as the refractive powers of diamond, jargon, ruby, garnet, pyrope, sapphire, tourmaline, rubellite, pistazite, axinite, cinnamon stone, chrysoberyl, and chrysolite, exceed it. If an object be viewed through two polished and inclined surfaces, of any substance supposed to be one of these minerals, when plunged in oil of cassia, the substance is a paste or artificial stone if the refraction is *from* the point to which the surfaces are inclined, and a real mineral when the refraction is made *towards* that point. The soundness and purity of glass for lenses may be ascertained in a similar manner.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Stocker, of Martock, in the county of Somerset, gunsmith, for a cock made of metal and wood, for drawing liquor from casks, which produces stop superior to that which is effected by common cocks, and prevents the liquor from coming in contact with the metals, except when the liquor is in the act of being drawn, and is running from the cask.—10th January, 1814.—2 months.

To

To John Duffy, jun. of Ballsbridge, near Dublin, callico-printer, for his method of producing patterns on cloths, made of callico or linen, or both, by preserving or defending mordants or colours previously applied to them from injury, when it is required to pass such mordants or colours through solutions of acids, of acid salts, of metallic salts, or of combinations of the oxymuriatic acid.—8 February.—6 months.

To Timothy Harris, of Foley Place, Portland Chapel, in the county of Middlesex, Esq. for a machine or machines for ploughing, laying on colours, called grounds, printing flocking, and pressing, so as to produce an even smooth face upon paper, silk, linen, woollen, cotton, and various other articles.—8th February.—2 months.

To John Vallance, junior, of Brighthelmstone, brewer, for his apparatus for the certainly cooling brewers', vinegar-makers', and distillers' worts, wash, &c.—8th February.—6 months.

To John Kershaw, of Glossop Dale, in the county of Derby, cotton spinner, and John Wood, of the same place, gentleman, for their mode of preparing flax for the purpose of being spun on the like machinery as cotton.—10th February.—2 months.

To Joseph Bramah, of Pimlico, engineer, for his method of applying certain species of earth, which will prove useful and be found productive of great public benefit, inasmuch as it will, when so applied, prevent, destroy, and finally extirpate what is called the dry or fungus rot; and will serve as a substitute for lead, in making of oil paints.—10 February.—2 months.

To William Francis Hamilton, of Asylum Buildings, Westminster Road, engineer, for certain improvements in optical instruments and apparatus.—12 February.—2 months.

To Richard Price, of Bristol, ironmonger, for his improved cooking apparatus.—12 February.—6 months.

To John Buddle, of Walls End, in the county of Northumberland, gentleman, for his fire-pan or fire-lamp, in which small or inferior coals may be consumed in the place of large or round coals; and hath also invented a fire-grate to be fixed at the bottom of the chimney in the ordinary mode, in which fire-grate or fire-stove small or inferior coals may be consumed on all occasions, and for all the same purposes as larger or round coals. 21 February.—2 months.

To James Thomson, of Colebrook Terrace, Islington, for certain improvements in the construction of fire-arms, and the locks to fire-arms.—9th March.—4 months.

To Daniel Goodall, of Burton Latimer, in the county of Northampton, for the manufacturing of English crapes from silks, dyed and coloured, both before and after they are thrown or spun

spun into silk, or silk for the manufacturing of crape, and introducing weavings or working into the warp and shute of such crapes, blacks, white, coloured, and fancy silks; and also black, white, coloured, and fancy cottons and worsted, and also gold and silver, and every other description of plain or fancy materials. —14th March.—6 months.

Meteorological Observations made at Cambridge, from February 14, *to March* 15, 1814.

Feb. 14.*—Cold northerly wind, some irregular *cumuli.*

Feb. 15.*—Very clear sky all day, with east wind.

Feb. 16*.—Clouded over early; then it cleared, and afterwards *cumuli*, of light flimsy texture, formed and passed gently along. Bright clear night, and frosty.

Feb. 17.—Clouded early, then flimsy *cumuli*, followed by a clear frosty night, and high barometer.

Feb. 18.—Cold easterly wind in the morning, clouded and westerly wind by night, with small rain.

Feb. 19.—Cold north wind, light flimsy clouds: bright starlight night and hard frost.

Feb. 20.—Perfectly clear and calm day, but cold and a gentle wind of easterly and variable direction.

Feb. 21.—Frosty and cold early; the formation of *cirrocumulus*† aloft, indicated increased temperature, which followed, with a clear night; wind southerly.

Feb. 22.—Clear still day, easterly wind, and a few flimsy *cumuli.*

Feb. 23.—Clear day, easterly and southerly wind, but very calm: very bright nights of late.

Feb. 24.—Cold morning, light *cirrus* approximating to *cirrostratus* spread aloft, fibrous, flimsy and variously mixed‡.

* The observations on the 14, 15, and 16, were made between Hackney and Cambridge.

† The *nubeculæ* composing this cloud were large and separated, and expanded and became flimsy and looser afterwards. In some places rows appeared approximating to *cirrostratus.*

‡ I noticed an electrical phænomenon this morning not very usual with me. On pulling off a great coat from a flannel gown underneath, I heard the snapping of and also discerned numerous electrical sparks of great magnitude between the two aforesaid garments, which shews the high electric properties of flannel capable of throwing even some modifications of woollen into an opposite state.

Feb.

Feb. 25.—Clear cold frosty weather, and very bright night. Wind E. SE.

Feb. 26.—The same clear cold weather, without a cloud, continues. Wind gentle, E. and SE.

Feb. 27.—Clear cold day. *Cirrus* approaching to the nature of *cirrostratus* aloft.

Feb. 28.—Clouded, with warmer air, and falling barometer; some small rain towards night. Moon seen at times, with some irregular appearances of *cirrus*, *cirrostratus* of the rounded *cirrocumulative* form, and of *cirrocumulus*. Barom. down to 28.82.

March 1.—Rainy day, and warmer; clear night.

March 2.—Snow showers; light flimsy clouds by night. Barom. at midnight so low as 28.80.

March 3.—Barom. rising again; cold and unpleasant day.

March 4.—Cold raw wet day, wind easterly and snowing.

March 5.—The snow lay thinly, and continued to fall; most unpleasant easterly wind.

March 6.—The same sort of weather; clear by times.

March 7.—Snow on the ground and falling, E. and N.

March 8.—Cold wet snowy weather, easterly wind.

March 9.—The same snowy wet weather, E. and N.

March 10.—Very small spiculæ of snow falling early, when the blue sky appeared bright above it; very wet snowy unpleasant day. Easterly.

March 11.—Snow still lays and falls by times. E. N. NW.

March 12.—Cold cloudy day. E. wind.

March 13.—Cold and cloudy with snow. N. and E.

March 14.—Clouded and cold, raw and frosty air. E. and variable.

March 15.—The same cold frosty air, with snow on the ground, and E. wind.

The present period has exhibited an example of long continued frost and snow, which has rarely happened this time of year, in the memory of persons now living. Abundance of sea fowl have been shot in the marshes. A few days ago a man at Wisbeach, near Ely, shot four swans out of a large flight passing over that place on the wing. They appeared to be the wild swans, (*Anates Cycni feri*) for, in one which I dissected, I found the *trachea* entering the hollow of the *sternum*, and reflected again over it into the chest.

Bennet College, Cambridge,
March 15, 1814.

THOMAS FORSTER.

METEORO-

METEOROLOGICAL RESULTS

Of the Pressure and Temperature, deduced from diurnal Observations, made at Manchester, in the year 1813. By Mr. THOMAS HANSON, Surgeon.

Latitude 53° 25′ North, Longitude 2° 10′ West of London.

1813.	PRESSURE.					TEMPERATURE.				
	Mean.	Max.	Min.	Range.	Greatest Variation in 24 Hours.	Mean.	Max.	Min.	Range.	Greatest Variation in 24 Hours.
January, ...	30,107	30,75	28,85	1,90	,50	35°,66	51°	22°	29°	19°
February, ...	29,536	30,50	28,40	2,10	1,00	40,97	54	32	22	17
March, ...	30,196	30,70	29,37	1,33	,75	44,60	57	26	31	20
April, ...	29,909	30,54	28,65	1,89	,70	46,45	66	29	37	28
May, ...	29,641	30,20	29,10	1,10	,55	54,66	72	38	34	18
June, ...	30,060	30,48	29,40	1,08	,40	58,75	77	41	36	26
July, ...	29,791	30,41	29,24	1,17	,45	62,36	83	44	39	25
August, ...	30,111	30,57	29,30	1,27	,50	59,54	75	42	33	24
September, ...	30,083	30,55	28,50	2,05	,95	55,91	68	41	27	26
October, ...	29,613	30,47	28,24	2,25	1,15	46,64	65	29	36	20
November, ...	29,767	30,54	28,85	1,69	1,55	40,80	53	28	25	14
December, ...	29,892	30,75	28,84	1,91	,61	37,60	51	24	27	17
Annual Means,	29,900					48,66				

RESULTS OF RAIN, BAROMETER, AND WIND.

1813.	RAIN.			EVAP.	BAROM.		WIND.										
	Inches.	Wet Days.	Upon Blackstone Edge.	Inches.	Spaces.	Changes.	NORTH.	N. East.	EAST.	S. East.	SOUTH.	S. West.	WEST.	N. West.	Variable.	Brisk.	Boisterous.
January, ..	1,445	6	——	——	8,10	19	0	5	6	5	5	1	1	2	6	2	0
February, ..	2,040	14	——	——	13,15	21	0	0	0	0	7	12	7	1	1	12	5
March, ...	1,490	11	——	——	7,80	19	5	1	0	1	9	3	10	1	1	7	0
April, ...	,655	7	4,215	——	6,90	18	2	5	2	1	1	7	8	2	2	7	3
May, ...	7,140	22	14,890	2,585	7,00	21	0	5	0	1	9	8	6	0	2	2	1
June, ...	1,935	9	4,520	3,285	4,95	12	1	3	4	4	1	4	6	3	4	2	0
July, ...	3,440	12	5,430	3,075	6,10	11	0	1	0	0	6	13	5	4	2	0	0
August, ...	2,425	12	4,840	2,665	5,04	15	3	3	0	1	1	13	6	4	0	0	0
September, ..	3,008	10	9,840	2,040	6,60	13	1	5	1	1	4	9	1	7	1	1	1
October, ...	5,795	10	19,220	1,810	10,15	22	0	10	0	4	0	14	1	2	0	1	0
November, ..	4,525	11	11,550	1,160	10,00	13	0	3	0	7	2	8	9	0	1	6	0
December, ..	1,005	9	11,580	,646	10,04	7	4	4	1	7	3	7	1	2	2	2	0
Total....	34,903		86,085	17,266	95,83	191	16	45	14	32	47	99	61	28	22	42	10

JANUARY.—The commencement of this period was mild, cloudy, and humid; the wind being for the most part south; rain fell in six instances, at intervals, to the 13th; when there was a slight fall of snow, for the first time:—an easterly wind diminished temperature, and dry atmosphere, were

were now the leading occurrences to the end:—the minimum temperature of 22° was on the night of the 25th.

FEBRUARY—Was decidedly a warm month, as it was attended for the most part with a south and south-west wind, but which blew very strong; on five days hurricanes occurred, they blew chiefly from the south-west quarter, and were attended with frequent showers of rain.—On the 13th, there was a faint lunar *halo;* and on the 23d, hoar frost.

MARCH —The first ten days were mild and warm, with a few showers of rain; but the tempercture experienced a sudden depression on the 12th; this arose from a change of wind from west to north, but its continuance in that quarter was of short duration; for the monthly maximum was on the 18th, being an augmentation of 31°.—Rain, with slight showers of snow, closed the month.—Wind south, and west, on nineteen days; its strength never reached a hurricane.—Upon the whole, the weather was favourable to vegetation.

APRIL.—Was ushered in with a low pressure, and temperature; the latter showed its monthly minimum on the fourth; previous to which, there were several showers of snow, and hail, and peals of thunder; which were succeeded by a quick augmentation of temperature, as well as a gradual one of pressure.—On the 10th, the weather became so serene, warm, and brilliant, that the thermometer indicated a summer's heat, being as high as 66°, which was an increase since the fourth, of 37°:—vegetation, of course, made a rapid progress, but being too early, a check might be expected; accordingly the last ten days were marked with frequent showers of snow and hail, and boisterous north and north-east winds, which did great damage to vegetation, particularly to tender buds, and foliage in exposed situations.—Blossoms of fruit trees, &c. were never known to be more promising, but the severity of temperature, and hail-storms, particularly of the strong east winds, almost stripped them of their beauty.

MAY.—Although there was a gradual increase of heat, from the commencement of this period, yet the prevailing easterly winds had not ceased to be destructive till about the seventh, when the weather became more mild, and nature seemed once more eager to repair the injury done to trees and vegetation.—Rain about this time was much wanted, as the fall in the two preceding months had scarcely exceeded two inches in depth.—From the seventh to the twenty-sixth, rain fell daily, with the exception of the twelfth, sometimes in very heavy and long continued showers, and in four instances with thunder and lightning. On the 24th, a hail-shower:—this period was generally favourable to the productions of the earth.

JUNE.—In two instances the diurnal temperature was lowered to 50°, the first was on the sixth, and was in consequence of an easterly wind; the latter was on the 19th, and which was immediately preceded by six days of almost incessant but gentle showers of rain. On the 13th, a shower of hail. This month was frequently marked with brilliant days, which, with the rain, were very seasonable.

JULY—Was remarkable for much thunder and lightning, interspersed with showers of rain, and in two instances hail.—On the 30th, after a high but desultory state of temperature, there was a sudden augmentation of 32°, being as high as 83°:—the monthly minimum of 44°, occurred on the third, being a difference of 39°.

AUGUST.—The first twelve days of August were cloudy and rainy, which had the effect of lowering the temperature, for on the 24th the minimum was as low as 42°.—The force of evaporation obeys the vicissitudes of temperature; in the present instance, the monthly quantity is four-tenths of an inch less than the evaporation for July.—Neither thunder, lightning, or hail, occurred; and there were few changes of atmospherical pressure, but the two principal ones commanded great ranges.

SEPTEMBER

SEPTEMBER.—The weather for the first fifteen days, was very gloomy, cloudy, and wet, with an unsettled state of temperature.—In about sixty hours, viz. from the 7th to the 10th, there was a loss of 27° of temperature, when it became more settled; with a brilliant serene atmosphere, and a high barometrical pressure, which continued to the end.

OCTOBER.—On the fourth, the temperature was at the monthly maximum, when rain fell very copiously; the temperature now continued to descend to the 18th, when freezing was observed the first time this season.—The heat soon after rose, and the weather to the end was fine and dry, with the exception of the two last days.—Prevailing winds, south-west.

NOVEMBER.—The most prominent variation in this month was, the vibratory impulse given to the atmospherical pressure during the first half of the month, indeed a similar occurrence took place at the same time with the temperature. The weather upon the whole was mild for the season, as the temperature was very seldom under freezing.—Rain fell copiously from the 8th to the 18th.—No hail was noticed, and there was only one appearance of snow.

DECEMBER—Was decidedly gloomy, cloudy, and rainy; but not so cold as is usually the case at this time of the year; except the few last days, the nightly state of temperature, (in consequence of a continuance of a gentle north wind) was lowered upon three instances, eight degrees under freezing.

The annual barometrical pressure for the past year, is 29,900 inches. The maximum of 30,75 occurred twice, viz. on the 22d of January, and the 26th of December; the minimum of 28,24 inches, was on the 17th of October; the range of the two extremes of course will be 2,51 inches.—The greatest variation in twenty-four hours for the whole year, was on the 14th of November, being 1,55 inches.

The mean annual temperature is 48°,66, being half a degree more than the annual temperature of 1812; the maximum was on the 30th of July, and the minimum on the 26th of January; the difference of the two extremes, will make a range of 61°. Greatest variation in twenty-four hours was 28°, which occurred on the 14th of April.—The mean temperature of the six summer months is 56°,28, and for winter 41°,04.

The annual fall of rain, snow, hail, &c. is near 35 inches in depth; Mr. George Walker's account of rain, is two inches more, but the low situation of his gauge, compared with mine, will partly account for the difference. I am enabled to state, that May, October, and November, were the wettest months of the year, and April the dryest. I have been favoured with an account of rain, taken at *White Holme Reservoir*, upon *Blackstone-edge*, by my friend Mr. Matthew Leadbetter, of Lane-Side, near Middleton, a gentleman whose accuracy of observation I can place implicit confidence in; his account commences on the first of April, as will be seen in the above table; the very great fall of rain upon Blackstone-edge is most astonishing, being 86 inches in depth, in nine months.—At a future opportunity, I intend to give a description of the place where the gauge is fixed, at present it may be proper to observe, that the funnel is exactly the same diameter as mine, and the same means are used in measuring the quantity collected. From so remote and elevated a place, situated as it is upon the borders of the two counties of Lancashire and Yorkshire, perhaps where no observations of the kind were ever before made, it cannot fail of being particularly interesting, but in an especial manner to the Proprietors of the Rochdale Canal, as their hopes of supply are principally drawn from that source.

The south, south-west, and west winds, have been the prevailing ones.—The most brisk and boisterous winds blew in February, March, and April.

Total quantity of water evaporated (from a surface of water exposed to the effects of winds and the sun, but not to its direct rays,) since the first of May, is a little more than 17 inches.

Meteoro-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For March 1814.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Feb.	26	26	37	25	30·04	0	Fair
	27	25	37	28	·00	0	Fair
	28	32	41	40	29·50	0	Cloudy
March	1	40	40	30	28·90	0	Rain
	2	30	38	37	·85	0	Snow and Rain
	3	37	40	32	·99	0	Cloudy
	4	32	35	30	29·28	0	Snow and Rain
	5	30	32	28	·60	0	Snow and Rain
	6	29	38	28	·85	0	Fair
	7	30	30	24	·72	0	Snow
	8	26	33	28	·73	0	Cloudy
	9	28	33	28	·62	0	Foggy
	10	29	33	30	·50	0	Snow
	11	28	36	28	·70	0	Cloudy
	12	28	37	29	·74	0	Snow
	13	28	36	32	·87	0	Cloudy
	14	29	35	33	30·20	0	Cloudy
	15	29	33	32	·38	0	Cloudy
	16	33	37	27	·38	0	Cloudy
	17	30	34	27	·28	0	Cloudy
	18	28	31	28	·20	0	Cloudy
	19	27	29	35	29·99	0	Cloudy
	20	35	46	42	·78	7	Fair
	21	43	47	41	·70	0	Rain
	22	44	50	43	·78	12	Fair
	23	42	51	40	·82	13	Fair
	24	40	53	44	·70	0	Rain
	25	44	54	45	·69	0	Rain
	26	46	55	45	·72	9	Fair

N. B. The Barometer's height is taken at one o'clock.

L. *Influence of atmospheric Moisture on an Electric Column composed of Discs of Zinc and Silver.* *By Mr.* THOMAS HOWLDY, *of Hereford.*

Hereford, March 24, 1814.

SIRS,—EARLY in the spring of last year, I discovered that the regularity of the action of Mr. De Luc's Electric Column was much affected by the varying state of moisture in the atmosphere; and in consequence of this discovery, previous to the 19th of April, I had repeatedly exposed an instrument of the kind to the rays of the sun, as well as to the warmth of a fire, in order to dissipate from the surface of the glass tube the film of moisture which that substance when unvarnished invariably acquires from the air, except when the latter is in a very dry state. During the succeeding summer and autumn, as well as the *present winter*, I have often observed the alternate effects produced on the column by a moist atmosphere and a dry one. As the quantity of moisture in the ambient air increases, a greater deposition of it takes place on the surface of the glass tube; and in proportion as this effect is produced, the insulating faculty of the glass is impaired, and the electrical phænomena of the column are diminished in intensity; because the electric fluid, which would otherwise accumulate at the zinc end of the column, seizing on the moisture, and by its means traversing the exterior surface of the tube, restores, either partially or entirely, between the extremities of the column, the equilibrium which its electromotive power has a tendency to destroy. But in proportion as the ambient air grows drier, the film of moisture which had been deposited on the surface of the glass tube is lessened; and consequently its resistance to the passage of the fluid is increased: an accumulation of electric fluid proportioned to that increased resistance, therefore, takes place at the zinc extremity of the column; and consequently the intensity of its electrical phænomena becomes augmented.

Experiments evincing the Influence of atmospheric Moisture on the Action of an Electric Column consisting of one thousand Discs of Zinc and Silver, of a Construction similar to those described in Volume xxxv. *p.* 84, *of Nicholson's Journal; made the 6th February* 1814.

Series 1. The positive extremity of the column resting on the cap of an electrometer, the negative communicating with the ground; the gold-leaves diverging about a quarter of an inch. The extremity of the glass tube at the negative end of the column was touched with a finger, and the leaves instantly closed. They opened again very slowly; and when they had attained their former divergence, the tube was touched at the positive end, and the leaves closed as before. After they had opened again to their

their former extent, the middle of the glass tube was touched, and the leaves again closed. The leaves having regained their original divergence, the waxed extremities of the tube were touched in succession, the leaves experienced a diminution of their divergence each time, but did not quite close.

Series 2. The wire of communication was removed from the negative end, by which the column became insulated: after waiting some time, the utmost divergence that the leaves attained was scarcely one-eighth of an inch; the tube was then touched as before, and the leaves instantly closed; this was repeated several times with the same effect.

Series 3. The column was now taken and held to the fire for about four or five seconds only, every part of it being exposed to its heat. It was then placed as described in Series 1, and the gold-leaves opened to the extent of three-fourths of an inch. The glass tube was then touched in various places, as well as its waxed extremities; but no decrease in the divergence of the leaves ensued; nor did any ensue when the contact was continued for several seconds. The communication with the ground was removed as in Series 2, and the touching repeated; but the leaves remained unaffected.

The column was left thus insulated for about six minutes; on returning to it at the end of that time, the leaves of the electrometer were seen diverging as in Series 2, about one-eighth of an inch; and on touching the glass tube they immediately closed, so quick had been the deposition of moisture on the glass.

At the time the foregoing experiments were made, there appeared to be abundance of moisture in the air; the sky was covered with a continuous sheet of cloud, which was depositing moisture in drops so small as to be invisible; and the streets and all objects out of doors were very wet.

Series 4. Made on the 14th instant, when the atmosphere was in a drier state than in the former instance. The column was in the position described in Series 2, and the gold-leaves diverged three-eighths of an inch. The glass tube was touched, and the contact continued for a second or two, but the leaves experienced only a very slight diminution of their divergence. The negative end of the column was now made to communicate with the ground by putting a finger upon it; the gold-leaves doubled their divergence almost directly: the finger being removed, the glass tube was touched, and the leaves were immediately reduced to their original divergence of three-eighths of an inch. The same effect followed when the negative extremity of the column was kept in the hand while the tube was touched.

The column was next presented to the fire for two or three seconds; and when its extremity was replaced upon the cap of

ther

the electrometer, the leaves opened instantly nearly to their full extent; and though the finger was put upon the tube in various places, the leaves struck, and continued to strike, the sides of the electrometer.

For a considerable time, during which a great number of experiments similar to the above were made, the results obtained were uniformly the same as the preceding, except in two or three instances which occurred when the column was insulated, and the leaves of the electrometer had only a small divergence: in those instances the leaves seemed to *increase* rather than diminish their divergence when the glass tube was touched; especially when the finger was laid upon it near its negative extremity: but the difference of divergence being exceedingly small, I was at first inclined to think that my sight had deceived me. I determined, however, should the apparent anomaly recur in any future experiment, to ascertain whether it were real or fantastical; but some weeks elapsed before I had the opportunity of decisively establishing its reality. The subsequent experiments made on the 18th of the present month exhibit this remarkable fact distinctly, and show that opposite effects may sometimes be produced by the same cause.

The column had been in the position as in Series 2, for several hours; the leaves of the electrometer diverged about $\frac{1}{10}$th of an inch. The glass tube was touched near its middle with the finger; the divergence of the leaves was not lessened as is generally the case, but, on the contrary, seemed to be increased by it; the finger was therefore again placed upon the tube, and the contact continued; it then became evident that the leaves were opening wider, and in about two seconds their original divergence was fully doubled; the finger was removed from the tube, but the leaves still retained their divergence. As no alteration took place in the leaves, the finger was next laid upon the waxed extremity of the tube next to the electrometer; the leaves then very slowly lost their divergence, and finally closed. As soon as their separation was again perceptible, a finger was laid upon the glass tube near to its negative extremity; the leaves now opened more quickly than before, and presently attained a divergence of one-fourth of an inch. The finger was withdrawn from the tube, and after a few seconds was again laid upon the waxed extremity next the electrometer; the leaves gradually diminished their divergence; but before they quite closed, the finger was laid upon the opposite (negative) waxed extremity; the leaves began to open wider, and as the contact was continued, they soon regained their former divergence of one-fourth of an inch.

When the negative end of the column was made to communicate with the ground, the leaves attained a divergence of only

 five-

five-eighths of an inch in three minutes, and when the tube was simply touched that divergence was reduced to three-eighths of an inch. The column was then held to the fire for a few seconds; and on replacing it, and laying the finger on its negative extremity, the leaves struck the sides of the electrometer in less than half a minute. The finger being withdrawn, the column became insulated, and the strikings were prevented: the glass tube was then touched in various places, as well as its waxed extremities; but the divergence of the leaves remained unaffected by it, as they also did by a continuance of the contact.

To the above, the following observations and conclusions may not improperly be subjoined.

1. That when the air is tolerably dry, simply touching the glass tube is not sufficient to produce the greatest diminution of divergence in the leaves of the electrometer, but the contact must be continued for two or three seconds; and still longer if the waxed extremities be touched; and when the air is very dry, the diminution is scarcely perceptible by any continuance of the contact.

2. When the column is in an insulated state, and the divergence of the leaves but small; whether that divergence remains the same, or is increased by touching the glass tube; if a finger be laid upon the negative extremity of the column, the divergence of the leaves will be quickly augmented; and on withdrawing the finger, and *then* touching the tube, they will instantly be reduced to their original divergence.

3. In a few instances when the tube has been touched in a considerable number of places; the leaves, after opening to their former extent, have ceased to diminish their divergence when the tube was touched; the finger having partially removed from its surface the film of moisture, as well as the electric fluid which was creeping along it; but after waiting a few seconds, on renewing the touching, the same effect has followed as at first.

4. When the column has been exposed to the rays of the sun, or to the heat of a fire; if the moisture has been completely dispersed from its surface, no decrease nor increase in the divergence of the leaves will ensue when the finger is kept in contact either with the glass tube or its waxed extremities.

5. But after the moisture has been so dispersed, the electrical power of the column is always found to be augmented.

6. On the whole it may be fairly concluded, that the variation in the electrical action of the column is principally caused by the greater or less quantity of moisture adhering to its surface; according as the ambient air is disposed to impart moisture to that surface, or to take moisture from it.

The

The foregoing experiments and observations will, if I mistake not, place the electric column in a new point of view; for to its other singular properties may be added that of an *hygrometer:* they will likewise account in an easy, natural, and satisfactory manner for the phænomenon observed by Mr. De Luc, and described by him in Nicholson's Journal; without compelling us to have recourse to the hypothesis of "the production of a new quantity of electric fluid in the column by the sunbeams*."

The same may be said concerning the phænomena noticed by Mr. Singer when he placed an electric column, and a bell-ringing apparatus, before a fire†.

In the experiments alluded to, the *heat dispersed* from between the extremities of the columns the moisture which impaired the insulation; and thus enabled the electromotive power of the instruments to produce a greater accumulation and deficiency of electric fluid at their opposite extremities than could possibly exist while that moisture was interposed between them.

I am, &c.

THOMAS HOWLDY.

To Messrs. Nicholson and Tilloch.

LI. *An Attempt to determine the definite and simple Proportions, in which the constituent Parts of unorganic Substances are united with each other. By* JACOB BERZELIUS, *Professor of Medicine and Pharmacy, and M.R.A. Stockholm.*

[Concluded from p. 175.]

IV. GENERAL VIEW OF THE RESULTS OF THE EXPERIMENTS RELATED IN THESE DIFFERENT ESSAYS.

1.

WHEN two substances, which we consider as simple, are capable of being united in more than one proportion, these proportions, for a given quantity of the [positively] electrical body, are multiples by $1\frac{1}{2}$, 2, 4 . . . of the least proportion in which the [negatively] electrical body is capable of combining with it.

[We must not, however, understand this multiplication of proportions in the strict mathematical sense of the terms, which is equivalent to the involution of powers, but simply as relating to the multiplication of the quantities by the numbers in question. *Gilbert.*]

There are however many phænomena which make it probable, that the multiple $1\frac{1}{2}$ is only apparent; and hence it follows, that the combination, from which it is deduced, does not exhibit the

* Nicholson's Journal, vol. xxxvi. p. 315. † Ibid. p. 374.

true minimum, but that there are other lower proportions, according to which it may be a multiple by 6, 12, or 18. Thus, for example, the arsenic acid contains once and half as much oxygen as the arsenious; but the black oxide of arsenic, which is formed by the oxidation of the metal in the open air, contains only one-fourth as much oxygen as the arsenious acid; and hence the oxygen of this acid is truly a multiple by 4, and that of the arsenic acid by 6, of the oxygen in the black oxide. I have shown how a similar remark is applicable to the acids afforded by sulphur.

The progressions hitherto discovered are expressed in even numbers. The only exception is the progression for the combinations of oxygen with ammonium, which, if we set aside the possibility that hydrogen may be an oxide, will advance in the following irregular manner: 1, $1\frac{1}{2}$, 3, $4\frac{1}{2}$, 6, $7\frac{1}{2}$; that is, supposing the oxygen of ammonia to be unity. Hence we may infer that this series cannot begin from the true minimum, and that there must be lower stages of oxidation of ammonium, than nitrogen, one of which must probably be identical with hydrogen. If then the oxygen of nitrogen is a multiple of the oxygen of hydrogen by 6, 12, or 18, the series will become perfectly regular. I must also remark, that from the point at which ammonium changes its electro-chemical modification in forming nitrogen, the series proceeds with greater multipliers.

The relations, which are found between the component parts of more complicated substances, are all conformable to the laws which regulate the simpler combinations; for instance, the proportions of sulphur to iron in the sulphates.

2.

When two oxygenized bodies are combined, their proportion may be most readily determined from the quantities of oxygen which they contain, the one being always either equal to the other, or to an integral multiple of it.

To these combinations belong:

a. Salts. In *neutral salts* the oxygen of the acid is a multiple by 2, 3 . . 8 of that of the base. In the *supersalts* the number may be still higher. In the *subsalts*, the oxygen of the acid is sometimes a multiple of that of the base, sometimes equal to it, and not uncommonly an integral submultiple.

b. Hydrates, or *combinations of water: first, with acids.* In these the water takes completely the place of a base; the acid takes up a quantity of it which contains exactly as much oxygen as any other base with which it would be saturated: and this water is totally distinct from the water of crystallization contained by some of the acids.

Secondly:

Secondly: with bases. Here the water takes the place of an acid, but contains only an equal quantity of oxygen with the base, or sometimes even a submultiple only. The water of crystallization is here again distinct from the water combined in the place of an acid.

c. Combinations of alkalis, earths, and oxides, with each other in pairs. Although my experiments exhibit no example of such a combination, it is obvious that it must be classed in this place. For, whether we call the oxides of chromium, molybdænum, tungsten, tantalium, and tin, acids or oxides, the rule must be equally applicable to them, and that which is a law for one oxide must also hold good with respect to others. Besides, since such combinations of two bases are the foundations of double salts with double bases, and since the law prevails with respect to these salts, we may consider this as a sufficient proof of the general law.

Combinations of two acids belong also here, for example, the fluoboric acid, in which the weaker acid probably occupies the place of a base.

3.

When three or more oxygenized bodies unite with each other, the oxygen of the body which contains the smallest quantity is a common divisor, or rather an aliquot part of the oxygen of the others, each of which must therefore be an integral multiple of this smallest quantity.

To this law belong:

a. Salts containing water of crystallization. In the *neutral salts* the water of crystallization contains 1, 2, 3, 4, 5 .. times as much oxygen as the base, or more rarely $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$ as much. In *subsalts* in which the oxygen of the acid is a submultiple of that of the base, the water of crystallization contains a quantity of oxygen which is a multiple of both the quantities, in the acid and in the base.

b. Double salts. Examples of salts in which the oxygen of the one base is either equal to that of the other, or an integral multiple of it, are afforded respectively by the double salts of ammonia and magnesia, and by alum. When water of crystallization is present, the substance is a quadruple compound, and the oxygen of the component part, which contains the least of it, is a common divisor, or an aliquot part, of the oxygen of the others, as we have seen in alum, in which the oxygen of the potass is contained three times in the alumina, 12 times in the sulphuric acid, and 24 times in the water.

c. Combinations of several alkalis, earths, and oxides, including crystallized minerals. Although we are not authorized

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by any direct experiment related in these Essays to form a conclusion respecting crystallized minerals, there is however no doubt of its validity; for why should marble, fluor spar, and ponderous spar, be formed according to a law which is not applicable to other minerals? Such a diversity is scarcely conceivable.

On the other hand, the application of the rule will obtain for mineralogy a degree of mathematical certainty, and afford to chemists, who are engaged in the analysis of minerals, a ready mode of satisfying themselves, how far their operations are correct; while, without some such means, these analyses can never be brought to a sufficient degree of accuracy. It is true that, according to this view of the subject, the greater number of such analyses, even when performed by the greatest masters, can only be considered as approximations, which in many cases scarcely come at all near to the truth; but such is the ordinary course of human labours. Since Bergmann's analyses of salts, which were so masterly for his time, 30 years are scarcely elapsed, and yet many of them can at present hardly even be called approximations. And the numerical determinations of the proportions of mixture of various substances, which I have attempted to obtain with the greatest care, and with the utmost possible accuracy, and which I have inserted in this Essay, will certainly not be sufficient for the future; and it will be possible, with the assistance of the laws here developed, to correct them, and to bring them nearer to the truth. But I am confident that our successors will pardon the imperfections of my experiments; as it must be allowed that chemistry, in its present state of advancement, must still be grateful for the valuable labours of Klaproth, Vauquelin, and others, although most of their analyses are become insufficient for its purposes of progressive investigation.

4.

Lastly, when several combustible substances, which we consider as simple, unite with each other, the proportions, in which these combinations are possible, are determined by their capacity for oxygen, the combinations taking place in such a manner, that, if they were oxidated in a certain degree, the oxygen taken up by one of the substances, would be a multiple by 1, 2, 3 . . . *of the quantity taken up by the other.* Thus sulphur, phosphorus, and arsenic, unite with metals in such proportions, that a salt may be produced by the oxygenization of the compound, or at least would be produced according to the general rule.

The same is also true of other metallic compounds, which are separated from mixtures in a state of fusion, by chemical causes, such as crystallization or heat: for instance, of crystallized amalgams,

gams, or crystallized alloys, which are sometimes obtained in purifying metals by assaying them. The union by fusion is analogous to the solution of a salt in water; and may be effected in almost all proportions; but when a salt crystallizes, a definite combination with water takes place; and when crystalline alloys are formed in the gradual cooling of mixtures, the part, still fluid being poured off, the crystallized part exhibits a fixed and definite combination. When a combination of two metals is capable of affording saline bases by oxidation, each part, in its conversion into a protoxide, commonly takes up the same quantity of oxygen. The *arbor Dianæ* affords an example of such a combination which is easily examined. When a compound contains several metals, it may happen that some of the multipliers may be very large. It is difficult to obtain such combinations in a state of purity; and I must defer, for the present, the publication of the few experiments which I have hitherto made on them.

The combinations of some combustible substances with oxides, for instance those of sulphur, sulphuretted hydrogen, boracium, and telluretted hydrogen, with alkalis and alkaline earths, follow the same laws, as if they were combined with the metallic radicals of the alkalis or earths without oxygen; or as if they were united with oxygen, and entered into combination, as acids and oxides, with the alkalis or earths.

In these few lines, we have gone through the whole structure of inorganic nature, and have seen, how it may be reduced to a few very simple principles. Oxygen, the only absolutely [negative] substance in all nature, is every where the standard, by which the proportions between the component parts of all combinations may be measured. From the existence of such a common measure, it follows, as a necessary consequence, that compound bodies, when they mutually decompose each other, never, or at least very seldom, set at liberty a single atom of their component parts; that neutral salts, for example, decompose each other without losing their neutral condition; and that sulphurets decompose water without disengaging hydrogen.

It is unnecessary to expatiate here on the additional importance which chemistry gains, as a science, by such a reduction of its results to mathematical principles. This is however still but an inconsiderable step towards the mathematical perfection of the science; and it requires the united and powerful exertions of all chemical philosophers, to bring it nearer and nearer to the elevated rank, which it may be hoped that it will ultimately attain in the system of human knowledge.

LII. *New Outlines of Chemical Philosophy.* By Ez. Walker, Esq. *of Lynn, Norfolk.*

[Continued from p. 105.]

On Respiration.

Lynn, March 28, 1814.

Sirs,—It has been ascertained by philosophers who have written on the respiration of animals, that no air passes through the membrane of the lungs into the blood, for the same quantity of air that enters the lungs is returned again into the atmosphere, but its properties are changed. Messrs. Allen and Pepys have proved by a series of experiments, that "when atmospheric air alone is respired, no other change takes place in it, than the substitution of a certain portion of carbonic acid gas for an equal volume of oxygen *."

And Mr. Ellis observes, that "in man, as well as in the lower animals, the conversion of oxygen gas into carbonic acid constitutes the only essential change which the air of our atmosphere experiences in the lungs during its respiration †."

The modes in which the atmosphere is depraved by the living functions of animals, is a chemical process which has never been clearly explained. But from the preceding theory of combustion ‡, the changes induced on the air, and on the temperature of the blood, in respiration, will admit of an explanation which may not be deemed unsatisfactory.

Part of the thermogen which is contained in the air, taken into the lungs, passes through their thin membrane into the blood, in the same manner as the electric fluid passes through this or any other animal matter, and, meeting with photogen, generates animal heat, in a manner which will be more fully explained hereafter.

But the generation of carbonic acid gas in the lungs may be explained thus:

Let Q represent a quantity of oxygen gas contained in the lungs, and let it be divided into two parts, $a =$ the greater part, and $b =$ the less: then $a + b = Q$.

Let the thermogen in b be attracted into the blood by the photogen which it contains, and a will represent the oxygen gas remaining. Suppose a attract a quantity of carbon from the exhalant vessels of the lungs $= c = b$; then the whole quantity Q will become carbonic acid gas, when Q is divided according to the proportion of oxygen and carbon which a given quantity of that gas contains.

* Phil. Trans. 1809, p. 427. † D. Ellis on Atmospheric Air.
‡ Phil. Mag. vol. xlii. p. 367; and vol. xliii. p. 22.

According

According to M. Lavoisier, carbonic acid gas consists of 28 parts of charcoal united with 72 parts of oxygen, and that carbonic acid gas is composed of these two bodies combined in that proportion.

Example. Suppose $Q = 100$.

Then $a + b = 100 =$ the given quantity of oxygen gas; consequently $a + c = 100 =$ the quantity of carbonic acid gas expelled from the lungs in respiration.

$$\begin{aligned} a &= 72 \text{ oxygen.} \\ c &= 28 \text{ charcoal.} \\ \hline a + c &= 100 \text{ carbonic acid gas.} \end{aligned}$$

On Animal Heat.

I believe that it has never been explained, in a satisfactory manner, how animal heat is generated, although different hypotheses have been invented to account for this wonderful effect. It is, however, generally supposed that heat is generated in the lungs. Indeed, from the construction of the lungs, and the change which takes place in the air in respiration, there remains little reason to doubt of the truth of this supposition.

The internal surface of the lungs in man is estimated by Dr. Keil to be about ten times the external surface of the whole body, or about 150 square feet*. On this surface the blood is exposed in respiration; and part of the thermogen contained in the air being attracted by the photogen in the blood, heat is generated by their union, in the same manner as in all other processes which cause an increase of temperature. The heat thus generated is carried through the whole animal system by means of the circulation of blood, to supply that heat which is constantly flying off from the surface of the body.

On the Renovation of the Atmosphere.

It has been ascertained in the most satisfactory manner, that the atmosphere is depraved by the living functions of animals and vegetables, by combustion, and by various other processes; and yet it has been found by Dr. Priestley and other chemical philosophers, that the air in crowded cities contains as much oxygen gas as that of any other places. Dr. Priestley supposed that the purity of the air was preserved by the living functions of vegetables: but this hypothesis is highly objectionable; for the air is as pure in winter when all Europe is covered with snow, as in summer when vegetation is in the utmost perfection.

It appears, however, an obvious truth, that if the oxygen gas which is withdrawn from our atmosphere, by entering into new

* Tentam. Med. Phys. p. 80.

combinations,

combinations, were not restored to it again, the air would soon become unfit for the supporting of animal and vegetable life. And if the thermogen which enters the animal system, by means of the lungs, were not conveyed out of the body as fast as it is received, this invisible element would be constantly accumulating till it put a total stop to all the animal functions.

As neither thermogen nor photogen can be annihilated, we may infer, that the thermogen which is taken into the blood, after having entered into new combinations and passed through the animal system, flies off from the surface of the skin in combination with photogen. When this compound comes into the atmosphere, the thermogen is converted into oxygen gas by uniting with the moisture contained in the air; and the photogen, by the same means, becomes hydrogen gas, and ascends to the upper regions of the atmosphere, from whence it descends to the earth in a manner which will be described in some future communication.

The following facts are sufficient to show in the most satisfactory manner, that a strong attraction obtains between thermogen and moisture.

The electric machine never acts so well in a moist atmosphere as in a dry one, because the moisture attracts the thermogen (positive electricity) from the apparatus, and prevents its accumulation. And we experience more cold in a damp atmosphere, the thermometer being at 40°, than when the air is dry, though the thermometer may be as low as 25°. The reason is evident: the moisture in the air attracts the thermogen from the animal system faster than it is supplied by the lungs.

To Messrs. Nicholson and Tilloch. EZ. WALKER.

[To be continued.]

LIII. *Notes and Observations on the Tenth and Part of the Eleventh Chapters of Mr.* ROBERT BAKEWELL'S "*Introduction to Geology;*"—*embracing incidentally, several new Points of Geological Investigation and Theory. By Mr.* JOHN FAREY, *Sen., Mineral Surveyor.*

[Continued from p. 190.]

Notes, &c.

P. 231, l. 23, agitation of the waters*.—* A paper of Sir James Hall's, just printed in the Edin. Trans., treats very fully on this subject.

252, l. 25, artificial tarras*.—* The (late patent Cement of Parker, is made from the Clay-balls or Ludus Helmontii found in the London Clay, Rep. i. p. 111: a superior article

[P. 252] cle is now manufactured on Earl Mulgrave's Estate, from stone which was discovered to have these properties, by his Lordship's architect, Mr. William Atkinson, Rep. ii. p. 6.

Notwithstanding this, a very learned Wernerian periodical Writer, lately told one of his correspondents, who inquired the composition of this Cement—"If I recollect right, it is composed of clay, iron-stone, and lime, *beaten together;*"—instead of telling him, that the "clay-balls" are merely calcined to expel the water, and then pounded and ground to a fine powder, *without any admixture*, as is now so well known, and as the specification of Parker's patent, stated many years ago.

255, Plate IV. See pages 11 and 256 in Mr. B's volume.

255, l. 11, were it attainable*.—* Mr. William Smith long ago attained vastly greater "accuracy of detail" in his Geological Maps, as to nearly all the most important parts of England and Wales, P. M. xxxix. p. 425; Mr. Cary, in the Strand, has assured me that his Maps will soon be published, as mentioned in my 1st Letter, xlii. p. 42. Since these Notes were principally written, I have observed, P. M. xlii. p. 125, that Mr. B. acknowledges his *theoretic "classes and orders,"* only, to be within *his* power to delineate on Geological Maps, and that "minute accuracy of detail," "can only be applied to small districts or estates.!"

256, l. 22, alpine district*.—* This is extended from Devonshire much too far northward; and so in a less degree on the NW of Chester, and in Anglesea, see my 1st Letter, vol. xlii. p. 57.

257, l. 6, low district*.—* The extension of this is wanting in the Map, Plate IV. at its northern end and SW corner, as is pointed out in my 1st Letter. The former of these errors seems needlessly charged on Mr. Tuke, in page 266.

l. 10, middle district †.—† The extension of this is too great on the N of York, and W of Scarborough¶, and around and to the E of Exeter, and too little on the W and SW of Bath, &c. see my 1st Letter, xlii. p. 57.

258, l. 12, upper part of this Clay*.—* The alluvium on it, see my Note on p. 16, and 181.

l. 18, on the very spot †.—† The importance of the fact here related, (though with considerable obscurity) of an elephant's tusk being found *in* the upper part of the London Clay, has occasioned me to make several inquiries of persons resident in and near Shoe-Lane: and the results of these inquiries have been, that "the very spot" alluded to by Mr. B. was

¶ Since the above was written, I have observed, that a correspondent of Dr. Thomson's in his Ann. of Phil. ii. 148, seems almost to have persuaded him, that "transition limestone," occurs at Filley, SE of Scarborough, see P. M. xlii. p. 94.

[P. 258] was previously to the year 1800, covered by old and almost ruinous houses, near which, no Well, answering the description given by Mr. B. is known. "Near St. Andrew's Church," seems indeed, a loose and improper reference to Mr. Taylor's Printing Office, see Mr. B's p. 362: and no where in this neighbourhood could I hear tidings of any such event, as the finding of an elephant's tusk; perhaps Mr. B. may have met with the relation in some old magazine or newspaper, which had improperly mentioned a tusk, as found *in the Clay* of a deep Well, instead of *in, or on the gravel*, when beginning to sink such a one ?. See my Notes on pp. 16, 181, &c.

P. 259, l. 2, of Yorkshire *.—* P. M. xxxv. p. 130.

l. 4, Huntingdonshire and Rutlandshire †. — † Mr. B. would perform an acceptable piece of service, if he would point out the situations and proofs, of Chalk strata being "found below the surface," in either of the above counties. I believe the whole to be a mistake, which has often before been printed, as remarked in my Report i. 307 Note.—The Rev. Mr. Townsend in his "Character of Moses," p. 141, mentions chalk at Ridlington, in Rutland, and Stukeley, in Hunts.: I have been through both the Stukeleys in Hunts. which are on the great Clunch-clay, (Rep. i. 113) covered locally with alluvial clay, flints, small *chalk bolders*, &c.: and I have passed very near to Ridlington, in Rutland, on its S and on its E sides, and the same must be situated, on or near to the Northampton Sandstone, (Rep. i. 114): and not unlikely, has alluvial chalky Clay patches, such as I have noticed in many places in and near Derbyshire, Rep. i. 308, and of which I saw several instances between Leicester and Uppingham. It is greatly to be lamented, that Mr. T's work abounds with errors, wherever he attempts to speak of the facts of the British strata, beyond his own personal observations, and neighbourhood, in the west of England.

l. 22, Mr. Townsend has given ‡.—‡ In the preface to the work here quoted, Mr. T. acknowledges, being unacquainted with *the succession* or manner of tracing the strata, until *he learned the same from Mr. William Smith*, in 1801, (they first became acquainted in June 1799, I have been told), who before then had *completed his Map* of this part of the country, and had very freely shown the same, and communicated his facts to great numbers of persons, during the six or seven preceding years, Rep. i. 111, and P. M. xxxv. p. 114 N: in various parts of his work Mr. T. refers to *Mr. Smith*, (sometimes rather erroneously with regard to *Faults*, I believe) and here at least, Mr. B. might

[P.259] might have introduced his name, as hinted in my 1st Letter, vol. xlii. p. 58, as well as noticed the laudable endeavours towards ascertaining *the order and thicknesses of the strata*, by your able correspondent Mr. David Mushet, P. M. vol. xl. p. 50, and even by the Rev. Mr. Mitchell, Rep. i. 109 N. and P. M. xxxvii. p. 175 N, and xxxix. p. 94 N. In one point of view, we have all reason to be thankful to Mr. B, that our Names have not been coupled with such very erroneous statements, as are made in the following pages, and at p. 283, by Mr. B, as to the "strata containing Coal" (beneath the Lias and upper Red Marl) being only 700 yards beneath London!, and not deeper at Woburn, than some of the Newcastle Coal-pits!!. See the article *Coal* in Dr. Rees's Cyclopædia, written in 1805, where this subject has been considered.

262, l. 9, their known limits *.—* Here, and at page 284, is as bold an attempt to set up wild theory against just inferences, from undeniable and unvarying facts, respecting England at least, as ever was made:—all experienced Coal-miners know, that seams of Coal, or other *strata*, "never go out to the deep," or never terminate in the direction of their dip; except sometimes suddenly against a *fault*, and are then only thrown up or down, and are not "cut off," incomprehensibly, see a note on this subject in my 2d Letter, vol. xlii. p. 105. In one page we are recommended to expend the national Treasures, in search of objects, which in the next we are told do not exist,—perhaps a sly hit was here intended, at several wise ministerial expenditures, within the last 20 years.

263, l. 8, strata under chalk *.—* In one sense, this is undoubtedly true, of English Fullers' Earth, owing to the Chalk being almost at the top of our Series, but not in that here intended, in alluding to the Surry and Bedfordshire Fullers' Earth, because Red Marl produces a sort of this Earth, in different places, as at Brathwell, Rep. i. 465, and P. M. xxxix. p. 105, near Nottingham, &c. The neighbourhood of Bath also produces this Earth, if I mistake not, &c.

l. 24, with fossil wood †.—† The wood and animal remains here alluded to, I think, are found in the stratified blue Clay of Highgate, Hyde-Park, Brentford, &c.; but the large animal Bones were in the alluvium *upon this clay*, see my Notes on pages 16, 181, 258, &c.

264, l. 19, found east of it *.—* Query, see my 1st Letter, xlii. 57.

265, l. 22, expansion of the Basalt *.—* Except the large Whin-dyke across a very high part of these Moors, where

Coal-

[P. 265] *Coal-measures* occupy the surface, mentioned in my Note on page 108, and numerous large blocks of Basalt scattered pretty generally in the alluvium (derived probably, from the ruins of the eastern end of this Dyke, in the Sea), there seem no facts to show the existence of Basalt *under this district*, for the Alum Shale extends deeper than a boring could be made on the Coast near Mulgrave-Castle, (Mr. B. p. 266, and my Note thereon) and to the northward, to which the measures are rising, and Red Marl and Gypsum appear, near the mouth of the Tees; these I have formerly supposed to be separated by a great fault? P. M. xxxix. p. 95, and towards the bottom of the note in p. 96, have suggested, other queries respecting this Red Marl, one of which appears to me of greater importance, since reading an account of the strata in Gothland, in Dr. Thomson's Travels in Sweden; where, evidently, I think, the Danby-Dale Coal-field occurs, (and not that of Newcastle, as is there said) N of Hessingborg: the Coal-field near Boulogne, described in the 2d Edit. of Williams's Min. King. ii. p. 335, is with difficulty referred to the Danby-Dale, or to any other of our Coal-fields, from the particulars there given; it seems however to occur in the eastern end of our great south-eastern denudation, (P. M. xxxv. p. 130) below the chalk, but how far? it is extremely interesting to know, more accurately: perhaps some of your Readers can supply more particulars of this part of the French Coast, and of the southern part of Sweden, &c. Do not *these* Coal-measures also appear across the northern extremity of Jutland, north of the Viborg district? Do they not occur on the north-eastern side of Bornholm Island, in the Baltic? Do not lifted and denudated parts of these Coal strata appear at Rehburg on the NW, and Osterwald on the SE of Hanover? and again near Aix-la-Chapelle and near Liege? see M. De Luc's Geo. Trav. in France, &c. vol. i. pp. 342, 345, 365, 366, and 260; Kirwan's Geo. Ess. p. 305, and Parkinson's Organic Rem. i. 181. I have mentioned, P. M. xxxix. p. 100 N. the probability, that very thin seams of Coal, belonging to this field, have been noticed in the wells on the north side of Battel, in Sussex. Mr. Charles Smith, a Coal-master in Yorkshire, lately informed me, that about the year 1800, he sent some of his Colliers to Ashdown Park, in Sussex, to bore there for Coals, for his friend Thomas Bradford, Esq. and that in boreing 150 yards, they passed through nine thin seams of Coal! which probably therefore belong to this Danby-Dale Coal-field? So may the Coal in Portland Island, and at Kimmeridge, which I have not seen.

P. 266,

P. 266, l. 7, stone on *the* summit*.—* There are several summits to this extensive hilly tract, the southern and many of the highest of these summits, are occupied by the oalite Limestone of New Malton, as described in your xxxixth vol. p. 98, and the others northward, by Coal-measures of Danby-Dale, (P. M. xxxix. p. 100) or the Alum Shale from under them, probably answering to the Clunch Clay, Rep. i. 113, and P. M. xxxix. p. 100 N; and the same strata nowhere appear in Derbyshire, nor is it at all likely that they do so in Wharfdale, or other parts of the west Riding of Yorkshire.

l. 17, fifteen miles in breadth †.—† This is rather an incorrect description: the Alum Shale first rises on the Shore, SE of Stowbrow, at first rapidly, and then very easily, pursues the Coast NW (with local coverings of Red Alluvial Clay and Basalt Bolders, London Cherts, Flints, &c. &c.) turning some distance up each valley, before the Coal-measures *close over it*, all the way to Saltburn, a distance of 28 Miles; from hence the edge of the Alum Shale turns SW to near Whorlton and Osmotherly, a distance of 25 Miles more (making a length of 53 Miles, beset with Alum Works, ancient or modern), the Shale turning up the valleys as before, were the Coal-measures close over it: Barnaby-Moor NW of Gisborough, is also a large detached hummock of the Shale, I believe, but the breadth is nowhere considerable, of surface made by the Alum Shale, before the Coal-measures cover it:—unquestionably it underlies the whole of them.

The Coal-field E and SE of Thirsk, has certainly the same measures, I think, as Danby-Dale, and therefore the edge of the Alum Shale may be expected to accompany it on the west, in proceeding SW and S from Osmotherly, and if it cannot be so traced? the great *fault* which I have supposed to make an angle somewhere near Ainderby-Steeple, P. M. xxxix. p. 95, may account, why, in crossing a nearly flat country partly covered by alluvium, I believe, from the Alum-Shale (or Clunch Clay?) at Osmotherly, to Leaming (mentioned in my 1st Letter, vol. xlii. p. 54), we see no strata, perhaps, until the Red Marl between the Yellow Lime Rocks is reached, and we miss all the thick strata of the middle of the Series in the south-east of England.

Many whose knowledge of solid Geometry is scanty, and others who won't take the pains to study its application hereto (as I have done and explained in my Report i. 117), and others also, whose Theories tell them, that *there is no law* or regularity in stratification, and have but a slight

[P. 266] knowledge of its facts, will doubtless continue to cry out against these "imaginary faults" of mine.—They must be indulged herein, and we will continue our observations, until, as I hope, the Geological facts and structure of all England, are far better known than at present.

l. 25, by ascending the ravines ‡.—‡ The practical Men employed on this boreing, *knew better;* they had invariably seen, as they ascended the valleys, higher and higher beds of their Alum Shale, then the Doggers, then Gritstone and Shale strata, &c. belonging to the Coal-measures, each crossing in succession and being lost in the beds of the Brooks, instead of the stone there, *rising "from under the aluminous schist,"* which Mr. B. here tells them of!

Since Mr. B's work was published, and the above was written, it appears that he has again *travelled across this district*, and made some stay at Whitby, and I cannot avoid remarking here, that a part of Mr. B's time on this occasion, might have been more profitably spent, whether we regard his own reputation as an observer and writer, or the interests of science, in revising, "by a calm investigation of facts," and correcting, the completely erroneous description (as I know from an actual Survey of part of them), that he has here published, of the structure of these eastern Moorland Hills, (see P. M. xxxix. p. 97), than it was in addressing to you, his very ill-timed Letter, (P. M. xlii. p. 121), dated "Whitby, *July* 15, 1813," replying rather vauntingly, to a Letter of mine, that was not written until the day after this date, or published until the 31st of July!.

Mr. B. owes it, I think, to the *practical Men,* whose judgement or something else, he seems to have arraigned, merely on the authority of his *Hypothesis* of "the expansion of Basalt" beneath (p. 265), for making *an expensive boreing* on the shore, in search of what (according to Mr. B's Hypothesis) they might "by ascending the ravines" in the neighbourhood, have evidently *seen,* without further trouble or expense, that he should revise and correct his north-eastern Yorkshire observations, thus erroneously made and published to the world.

267, l. 1, coal 4 to 8 inches*.—* This is *Wood* Coal, thick enough in some places to be wrought, see my Note on p. 157. In your xxxvth vol. p. 257, an account of this Coal and the other strata, extracted from a fuller one published by Mr. Richard Winter, will be found, with some conjectures of mine thereon, made before I had the opportunity of examining this curious district.

267, l. 8 and 10, coal strata terminate †.—† At the Great Derbyshire

[P.267] Derbyshire Fault separating them from Red Marl, Rep. i. 165, and P. M. xxxix. p. 29: but the existence of which fault, Mr. B. questions at p. 212, see my 2d Letter, P. M. xlii. p. 105.

l. 11, but appear again ‡.—‡ There are reasons to doubt the identity of these Coal-fields, Rep. i. p. xiii.

l. 12, They are cut off **.—** This may perhaps be doubted, see my Note on p. 67.

l. 15, coarse breccia and gravel ††.—†† Red Marl strata of considerable width, separate these Coal-fields, see my Note on p. 67.

l. 20, red sand rock ‡‡.—‡‡ Red Marl, see my 2d Letter, P. M. xlii. p. 103.

268, l. 4 and 5, surrounded by red sand rock *.—* By Red Marl, see Phil. Trans. 1811, and P. M. xxxix. p. 28.

l. 8, South Wales †.—† See p. 299, Phil. Trans. 1806, Will. Min. King. 2d Ed. ii. 291, and P. M. xl. p. 52.—How far is the "gray greenish micaceous sandstone," or Pennard Rock, allied to the Basalt, greenstone, &c., found covering several other Coal-fields? see my Notes on page 285.

l. 22 and 23, primary and transition rocks ‡. — ‡ And probably they join them, near Exeter, see my 1st Letter, P. M. xlii. p. 57.

270, l. 7 and 8, hid by the alluvial soil *.—* P. M. xxxix. p. 30 and 95.

l. 21, Pontefract is built †.—† Rep. ii. 169.

271, l. 7, Halifax *.—* On the 3d Grit Rock.

l. 8, Huddersfield †.—† On or very near to the 4th Grit Rock, P. M. xxxix. p. 102.

l. 8, Sheffield ‡.—‡ On the 9th Grit Rock, Rep. i. 207, (Pond's Colliery).

272, l. 6 and 7, immediately upon Lime *.—* I have never visited "Pule Moss," but have been near "Stand Edge," on the south-west, and have minutes of the strata perforated by the Huddersfield Canal Tunnel, under the Grand Ridge, from two or three Agents or Workmen who were employed in the Tunnel; from which I understand, that the eastern end of the Tunnel begins in the 1st Coal Shale, near the bottom of which, a 15-inch Coal was cut, with a pretty rapid eastern dip, which was for some time wrought for the supply of the steam engines, then the thick 1st or Millstone Grit is penetrated, having the same dip, as had also the top of the Limestone Shale under it; but as the middle of the Tunnel is approached, the dip diminishes, the

[P.272] measures become flat, and then dip slightly westward. I had good reasons to think these representations correct, because after the Canal has descended many Locks to Wright's Mill, near the junction of the three Counties of York, Lancaster, and Chester, the vale of the Tame is deeply excavated in the L. Shale, extending thence S for 2¾ Miles, in which local denudation, I have mentioned, Rep. i. 236, that probably, the 1st Limestone is not far below the surface, in some parts.

The flat-topped Ridge in the measures, or *Strata-Ridge*, (Rep. i. 172), seen in this Tunnel, appears to range south-eastward under Goodgrave Edge, Woodhead, and Blakelow Stone, (where I believe the 1st Grit to have a greater elevation, upon it, than any other spot of ground in Derbyshire, see Rep. i. 5 and 21, and Mr. B. p. 278, and my Note thereon,) and to have occasioned the several *local denudations* of L. Shale, from near Padfield N, to within a Mile of Yorkshire, up both the Eden and the Longden valleys, in the deepest excavations in which, I have also suggested the probability, of 1st Lime being near at hand, Rep. i. 236.

I never before heard of the "Dykes of Limestone," mentioned here by Mr. B. p. 272, and shall feel much obliged if he will mention more precisely, the places, the number, the directions, inclinations, thicknesses, and qualities of stone ?, in these Dykes, so uncommon in this part of our Island.

l. 14, *argillaceous* Ironstone †. — † Rep. i. 232; in the draining of Mr. Joseph Gould's Meadows at Pilsbury, near to the great Limestone Fault, and 4th Limestone Mountain, (Rep. i. 286, 232, and ii. 393, and vol. i. p. 232 Note), similarly ribbed spheroids, or lenses rather, were found, but composed of Shale *Limestone*, instead of Ironstone; formerly they would have been called gigantic *Nuts*, but I hope these follies have sufficiently had their day.

My Friend Mr. Lawson, of the New Mint, lately mentioned to me, that while the Tunnel under Stand-Edge near Saddleworth, was driving, he saw there, numerous lenticular round nodules, one side of them more convex than the other, and having numerous concentric raised lines upon their surfaces almost as regular as if turned in a lathe; they were of various sizes, from 2 to 12 inches diameter, and he found them composed of *calcareous* Ironstone !.

l. 24, intersect *these* beds ‡.—‡ Quere, see P. M. xxxix. p. 103 and 100.

P. 273,

P.273, l. 7, red siliceous sandstone*.—* Red Marl and its occasionly imbedded Gritstone, see my 2d Letter, xlii. p.103.

l. 8 and 9, surrounded by this rock†.—† By Red Marl, see my Note on p. 268.

l. 10, in clay pits‡.—‡ In *alluvial* red and brown Marl Pits, Rep. i. 456.

l. 14, the red rock**.—** Gritstone imbedded in Red Marl, on which last (with very trifling exceptions, besides its imbedded gritstones), I travelled last summer from Shrewsbury to Bolton, on the N of Lancaster, as mentioned in my 1st Letter, v. xlii. p. 53—so far are the "shale (or shivers)," and the "Millstone Grit" of Whitehurst, and a "district of millstone grit and shale," from being real, which are shown in a Map, and mentioned in your xxxviiith vol. pages 270 and 275, or the incumbent "rock or shale which forms the basis of the district," p. 276, &c.—See also my Queries thereon at p. 237, which have not been noticed, that I am aware of.

l. 23, at Castleton††.—†† See my 2d Letter, vol. xlii. p. 113, and my Note on p. 48.

274, l. 1, the red sandstone of Chester*.—* Red Marl (and its imbedded Grit, Gypsum, Rock Salt, &c.), Rep. i. 147; and no notice is taken here, of the intervening Limestone Shale, 1st Grit and Coal-measures, shown in my Map, p. 97 of Rep. i. p. 172: unless, in the new terms of Mr. B. they are all "sandstone," see my 2d Letter vol. xlii. p. 103.

l. 4, limestone in Flintshire†.—† The improbable idea of some common Miners in Derbyshire, which I have often heard, and as is mentioned by Mr. B. at p. 220, that the individual Mineral Veins of Derbyshire, extend into North Wales, is not sufficient, or anything else which I have met with in Mr. B's work, to show the Limestone Rocks of these two distant Mining districts, to be the same. In your xxxixth vol. p. 427, I have suggested, that the Limestone Rocks underlieing Coal-measures in North and in South Wales, are the same, and I can now add, that I believe the same can be traced nearly through all the intermediate Country (as the upper of three Limestone Rocks, seen near Ludlow, as mentioned in my 1st Letter, vol. xlii. p. 53, and which I wish to name the "Halkin" Rock), with some local interruptions from Denudations, Faults, and Gravel coverings, and the same into and across Anglesea, proceeding south-westward.

[To be continued.]

LIV. *Facts and Observations towards a History of the Combinations of the yellow Oxide of Lead with the Nitric and Nitrous Acids.* By M. Chevreul *.

1. M. Proust having observed that the octahedral nitrate of lead, when boiled with laminæ of this metal, was converted into a yellow leafy salt, concluded that the lead was oxidated at the expense of the litharge, the base of the octahedral nitrate, and that consequently there was one oxide more at the minimum than the latter. Mr. Thomson, in a work upon Lead, resumed the examination of the yellow salt described by M. Proust: he was led by his experiments to regard it as a salt which differed from the octahedral nitrate only by an excess of base. In his System of Chemistry, Mr. Thomson renounced this opinion for that of M. Proust; but at the same time he observed that the quantity of oxygen in the oxide at the minimum differed very little from that of the litharge. He has not besides added any new fact, in order to prove the existence of a new oxide of lead.

2. At a time when the imagination and experience of chemists carefully examined the laws which preside over the combination of bodies, I have been surprised at the very little attention paid to a salt which might contain a new oxide, and raise to four the number of the oxides of a single metal. This consideration induced me to resolve the following questions:

Is there an oxide of lead less oxidated than litharge? If this oxide exists, what is the quantity of oxygen which it contains? and in what ratio is this quantity with that which constitutes the yellow, the red, and the puce-coloured oxides of lead?

3. The first inquiry which occupied my attention, was the analysis of the octahedral nitrate of lead; for I could not determine the proportion of oxygen which the lead absorbs in order for its solution in the nitrate, except by being perfectly acquainted with the proportions of the elements of this salt, since they furnished to the metal the oxygen which it required. I took octahedral nitrate, which had been crystallized, twice washed it with water, and reduced it to powder. I dried it by exposing it for several days to the sun, and afterwards heated it on paper. This nitrate was divided into several quantities scrupulously weighed, in order to be used in the various experiments about to be detailed.

4. I put five grammes of nitrate into a platina crucible weighing one ounce four drachms and 43 grains. I exposed it to a graduated heat, in order to reduce the salt to its base, and withdrew the crucible from the fire when no more nitrous vapours were extricated. I weighed it, and heated it once more red

* *Annales du Muséum d'Hist. Nat.* ann. x. p. 189.

hot

hot in order to discover if all the acid had been volatilized. When I perceived that there was no more diminution of weight, I found it necessary to put into the basin of the balance, in which the crucible was, 1·65 gr. in order to restore the equilibrium. This weight representing that of the acid contained in five grammes of nitrate, it follows that this salt is formed of

Acid	33	100
Oxide	67	203

A result which differs very little from that of M. Berzelius. This excellent chemist found

Acid	32·7775
Oxide	67·2225

5. Before commencing the examination of the yellow salt, I wished to know the action of the oxide of lead on the nitrate. I heated in boiling water equal weights of these two substances, and filtered the liquor in a flask while still warm. When the latter was full I closed it up, in order that the liquor might not come in contact with the carbonic acid of the atmosphere. Upon cooling, crystals of nitrate of lead were deposited in scales *. This salt has a slight saccharine and astringent taste: it crystallizes in scales or small needles. It is not acid. When we pass a stream of carbonic acid into its solution, we reduce it to octahedral nitrate, and to carbonate. Two hundred parts of nitrate heated in a platina crucible lost 39·72. Previous to decomposing this salt, I ascertained that no more water was extricated when heated in a close and very long glass tube. The nitrate is therefore formed of

Acid	19·86	100
Oxide	80·14	403

This analysis confirms the law laid down by Dr. Wollaston for the combination of the elements of salts, for in the nitrate of lead we find that the quantity of the base is double that which is contained in the acid nitrate.

6. The nitrate of lead differs so much from the yellow salt in respect of its physical properties, that it was impossible to regard them as identical, and to adopt the same opinion with Mr. Thomson; and what confirms the difference which existed between these two salts, is, that having prepared a little yellow salt, I found that it sent forth nitrous acid gas with nitric acid; whereas the nitrate did not emit any. From this moment I was of opinion with M. Proust that there was an oxide still more at the minimum than litharge.

7. In order to determine the quantity of oxygen in this oxide, I put into a matrass 350 grammes of water, four grammes of acid nitrate, and six grammes of lead cut in small pieces. I boiled the whole, and took care to pour boiling water into the

* I shall call this salt *nitrate of lead*, and the octahedral nitrate, *acid nitrate.*

 matrass

matrass as evaporation took place. The liquor gradually became of a fine yellow, and after two hours boiling the colour was at its maximum of intensity: subsequently it gradually diminished, and ended by entirely disappearing. There was deposited in the course of the operation a white substance which resembled hydrate or carbonate of lead. After twelve hours boiling, the nitrate apparently having no more action on the metal, I decanted the liquor hastily into a vessel, which I shut carefully: the lead when dried weighed 0·6 gramme: there was therefore 5·4 gr. of lead dissolved by 4 gr. of nitrate. This result is widely different from Mr. Thomson's, who says that 100 grains of nitrate can dissolve only 44 grains of lead.

8. It was easy to determine the proportion of the elements of oxide of lead at the minimum, since we knew the quantity of metal which had been dissolved by the acid nitrate. But two considerations prevented me from attempting it: in the first place, the yellow colour which the nitrate had assumed in dissolving the lead was certainly owing to the solution of this metal; but this colour having disappeared, was it not probable that the oxygen of the atmosphere was the cause of this discoloration? In the second place, the white matter which was deposited did it not proceed from the absorption of the oxygen or the carbonic acid from the air? In order to appreciate the influence of external agents, I repeated the experiment in the apparatus about to be described. In a matrass similar to that which had served for the experiment above mentioned, I put water boiled with four grammes of acid nitrate and six grammes of lead. I adapted to the matrass two glass tubes, one in the form of S, destined to replace the water which should be evaporated; the other doubly bent, which fitted into the upper part of a bell-glass filled with air. I had put into this bell-glass, which rested in a lime-bath, a glass containing a mixture of iron and moistened sulphur. In three days the oxygen of the air of the vessels having appeared to be absorbed as well as the carbonic acid, I lighted a fire under the apparatus, and the ebullition of the liquid was kept up fourteen hours without interruption. When the water began to boil it became yellow: *the colour fell away in two hours, and finally disappeared*. There was also, as in the foregoing experiment, *a precipitation of a white substance*. It was evident from this, that the oxygen and the carbonic acid of the atmosphere were not the cause of these phænomena. When the operation was ended, I allowed the liquor to rest, and decanted it in a flask which was made quite close. I passed water into the matrass, in order to detach the *white matter* from it, as well as the lead which had not been dissolved.

The latter when washed and well dried weighed six decigrammes,

grammes. The white powder was treated with weak nitric acid, it was dissolved without effervescence with the exception of a black powder weighing two centigrammes, which was nothing but sulphuret of lead. As this sulphuret formed part of the lead employed in the experiment, it is evident that it must be united to that which had not been dissolved: consequently the four grammes of nitrate had dissolved 5·38 gr. of metal. As the sides of the matrass were opaque, I washed them with weak nitric acid, and added the washings to the solution of the *white matter:* notwithstanding this washing they were always opaque. Finally, by inspecting them more closely, I ascertained that the glass had been corroded; and after evaporating the nitric solution to dryness, and taking up the residue by water, I obtained five centigrammes of silex. The part soluble in the water of the residue was nitrate of lead, which contained 47 centigrammes of yellow oxide: the *white matter* was therefore formed of *silex and hydrate of lead,* retaining perhaps a little acid.

9. The solution of acid nitrate of lead which was boiled over lead, and which had been decanted in a flask, deposited, after eleven hours, crystals in silky needles united into stars. They weighed 5·95 gr. after being dried. The liquor in which they were formed, concentrated without the contact of the air, gave at several times 2·71 gr. of crystals similar to the foregoing. There remained a mother water containing a little of this salt as well as of nitrite of potash; for the sulphuric acid made it emit a nitrous vapour, and the muriate of platina made an abundant precipitate in it of triple salt of potash. I separated almost the whole alkaline nitrite from the salt of lead by means of alcohol. This result strongly confirms the decomposition of the glass observed above, and seems to demonstrate that the 47 centigrammes of oxide contained in the *white matter* had been precipitated from the solution of the nitrate by the alkali of the glass which was dissolved.

10. To resume the facts of this experiment, and draw the consequence which must follow upon the hypothesis of an oxide more at the minimum than litharge, 5·38 gr. of lead were dissolved by 4 gr. of acid nitrate of lead which contained 2·68 gr. of litharge; but as there was 0·47 gr. of the latter precipitated, it is evident that the lead was not oxidated but at the expense of 2·21 gr. of litharge. Hence it follows, that by adding this quantity to the 5·38 gr. of lead dissolved, it is easy to ascertain the composition of the oxide at the minimum, since we know that 2·21 gr. of litharge contain 0·158 gr. of oxygen and 2·052 of lead. We find according to these data that 100 parts of lead ought to absorb 2·125 gr. of oxygen.

11. The

11. The small quantity of oxygen which the lead seemed to absorb in order to be oxidated at the minimum, and the reflection that this quantity did not follow any relation with the known oxidations of this metal, began to create doubts in my mind as to the existence of an oxide more at the minimum than litharge, and incline me to think that it was not improbable that the lead was oxidated at the expense of the nitric acid of the nitrate; that consequently the salt obtained from this operation was only a nitrite with a base of litharge; and that the nitrous acid extricated from this salt by the nitric acid (6) was merely separated from it, as when we pour nitric acid over an alkaline nitrite. What supported this opinion was: 1st, the nitrite of potash which I found in the mother water of the salt (9); 2dly, the litharge and the nitrous acid which this salt constantly yielded, whether I decomposed it by heat, treated it with acetic acid, or with carbonate of potash. It is true that it may be objected that in these decompositions the oxide at the minimum is re-oxygenated in the operation at the expense of the nitric acid which it converted into nitrous acid; but what weakened this objection was the observation I made of the non-action of the oxygen gas upon the solution of the salt. At first it seemed probable that, by boiling the lead with the acid nitrate, this metal only took from the nitric acid the quantity of oxygen which forms the difference between this acid and the nitrous acid: but the 5·38 gr. of lead having to absorb 0·41426 of oxygen in order to be converted into litharge, whilst the nitric acid of the nitrate could only yield 0·1557204 in order to be converted into nitrous acid, I concluded that there must have been a decomposition of water, or rather that a portion of nitrous acid was itself decomposed. This consideration induced me to make the following experiment, in order to collect the gas which might have been discharged.

12. I put into a matrass the same quantities of lead, acid nitrate, and water, which I had employed in the foregoing experiments. I adapted to it a tube doubly curved, the vertical branches of which were very much elongated. One of these branches, which entered the upper part of a bell-glass 15 lines in diameter, resembled a funnel. When the apparatus was well luted, the water of the bell-glass was three lines below the edge of the funnel. According to this arrangement, it was easy to see if there was any production of gas. I had given a great length to the vertical branches of the tube, in order that there should be the least possible quantity of water to be volatilized; and I had widened the branch which communicated into the bell-glass, in order that all the water which was evaporated might be collected in the tube, and not mix with that in the bell-glass. I made a fire

fire under the matrass, and took care during the whole operation to keep up a boiling heat only. By these means there was but very little water to be vaporized. The experiment began at seven in the morning; at eight the water was boiling. The air in the bell-glass was dilated. At nine the liquor was of a fine yellow. At half past ten *red nitrous vapour* was formed in the tube, and it augmented gradually. When it reached the bell-glass absorption took place, and a piece of turnsole paper which I introduced into it was strongly reddened. At eight in the evening the operation was stopped: it was evident that the air of the apparatus had been reduced into azotic gas, and that there had been a little nitric acid condensed in the tube. This experiment puts it beyond doubt, that it is not at the expense of the oxygén of the litharge that the lead is oxidated, but rather at the expense of that of the nitric acid; in the second place, that the nitric acid is reduced by the lead into nitrous acid, which remains in combination with the oxides, and into nitrous gas which is liberated. I am ignorant if the decomposition goes so far as to give out azotic gas.

13. This decomposition of the nitric acid fixed to one base is certainly very remarkable; and if actual experiments had not proved it, I could not have thought it possible. The acid nitrate of lead is not the only salt of its kind which is susceptible of being changed into nitrite; for, if we boil a solution of nitrate of potash over small pieces of lead, and if we concentrate the liquor so as that the greatest part of the nitrate is crystallized upon cooling, we find in the mother water plenty of *nitrite of potash*, which emits the red vapour when we mix sulphuric acid with it. This mother water contains merely an atom of lead, which sulphuretted hydrogen demonstrates. In this experiment, it is to all appearance the affinity of lead for oxygen and for water, which determines the decomposition of the nitric acid, while in the former it is the affinity of lead for oxygen and for the nitrous acid.

14. I have said above, that when we boiled over lead the solution of the acid nitrate of this metal, the liquor took a yellow colour, which ended by entirely disappearing. If we stop the operation when the colour is pretty deep, we obtain, upon cooling, yellow leafy crystals: the liquor from which they are deposited yields, when concentrated and cooled, crystals of the same kind, if the solution of the nitrate has not boiled too long over the lead. This salt is the same with that described by Messrs. Proust and Thomson, but it differs considerably from that which I obtained from the three experiments which I have mentioned: in fact, the latter does not colour the water which holds it in solution; instead

instead of being precipitated in yellow scales, it takes the form of pale-red needles which unite in stars. As this salt is only formed after the yellow salt, and with a greater quantity of lead than that which served to form the latter, it is evident that it ought to contain more base, and that it ought to be regarded as a subnitrite. Before explaining the properties of these two salts, I shall premise that it is very difficult (perhaps even impossible) to obtain perfectly pure nitrite by M. Proust's process: in fact, if we have not boiled the acid nitrate long enough over the lead, the nitrite may contain nitrate. Finally, if the ebullition has been too long, the nitrite retains some subnitrite, as appears from its colour verging towards red. The best process for preparing the nitrite consists in passing a current of carbonic acid into the solution of subnitrite, and in evaporating the liquor separated from the carbonate of lead. We then obtain crystals of nitrite, which we must press between bibulous paper to dry them: afterwards they must be exposed to the sun. Almost all my experiments were made with nitrite of lead produced by the subnitrite.

Examination of the Nitrite of Lead.

15. It crystallizes in yellow leafy flakes.

16. It is not very soluble in cold water: besides, its solution has but a slightly astringent and saccharine taste. 100 grammes of boiling water may dissolve about 9·41 gr. 100 parts of water at 23° of the centigrade thermometer, when mixed with 2 gr. of nitrite reduced into powder, dissolved 1·26 gr. after being 24 hours in contact. When we make a solution in boiling water, and when we cool it to 23°, the water retains more nitrite than it would have dissolved at the same temperature.

These determinations are not rigorous, because, when we dissolve the nitrite in water, there is always a portion of it decomposed by the carbonate of ammonia contained in the distilled water.

It appears to me, that when we crystallize several times the nitrite (coming from the snbnitrite) the first crystals obtained from it contain more base than those which are formed afterwards, and that the mother water of the latter was slightly acid. The first crystals were of a deeper yellow than the rest: they yielded when analysed one centieme of base more than the latter.

17. The solution of nitrite is yellow, it brings back to blue turnsole paper which has been reddened by an acid: it does not perceptibly absorb oxygen gas, at least after a contact of three days; when exposed to the air it is covered with a white pellicle of carbonate.

18. The

18. The sulphuric acid precipitates sulphate from it, it liberates a nitrous smell, but the red vapour is not very perceptible. It would seem that the latter remains in solution.

19. The nitric acid and the acetic acid, which form with the oxide of lead salts very soluble, emit the nitrous vapour when we project into these boiling acids the nitrite reduced into powder.

20. The carbonic acid gas when passed into the solution of nitrite of lead precipitates from it a part of the oxide in the state of carbonate. There remain in the liquor oxide of lead, nitrous acid in excess, aud carbonic acid. The carbonic gas which is not absorbed carries with it an atom of nitrous acid. I shall return to this decomposition of the nitrite by the carbonic acid.

21. The carbonate of potash decomposes it: there is a formation of nitrite of potash and carbonate of lead.

22. When we heat it gently it becomes clammy, and its colour is deeper. At a red heat it is reduced to pure oxide. The first portions of acid are extricated from it at the heat of boiling water.

23. The solution of nitrite, when boiled with yellow oxide of lead, is converted into subnitrite, but it requires a long time. When ebullition commences, the oxide, no matter how pulverulent it is, becomes flaky and white, and seems to pass to the state of hydrate. I am ignorant if it absorbs a little nitrous acid.

Examination of the Subnitrite.

24. It is of a pale red, inclining to yellow. It crystallizes in small silky needles which unite in stars.

25. 100 grammes of boiling water dissolved three grammes of subnitrite. 100 grammes of water at 23° of the centigrade thermometer dissolved about seven decigrammes of it after being 24 hours in contact with it. 100 grammes of boiling water, saturated with subnitrite and cooled to 23°, retain nearly 1·09 gr. of salt.

26. The carbonate of potash and the sulphuric, nitric, and acetic acids decompose it like the foregoing.

27. The carbonic acid when passed into its solution forms an abundant precipitate of carbonate of lead. The liquor becomes yellow by losing the oxide, and it contains oxide of nitrous acid in excess and carbonic acid.

28. The solution of subnitrite when poured into the nitrate of copper precipitates from it a powder of a greenish blue, which is a combination of the hydrates of copper and lead. I am ignorant if the acid, which I obtained from it by distillation, was essential to it, or if it was occasioned by my not having well

washed

washed it: the former of these opinions is the most probable. The nitrite of lead equally precipitates the nitrate of copper.

29. The solution of subnitrite forms with the muriate of gold a yellow flaky precipitate. If when the precipitate is nearly all collected we filter the liquor, it deposits metallic gold. I should almost be inclined to think this precipitate was a double muriate containing abundance of base.

[To be continued.]

LV. *On Electrical Phænomena; and on the new Substance called Iode.* *By Mr.* J. Murray.

Saffron Walden, Essex, April 12, 1814.

Sirs,—Some errata blend with my paper which you did me the honour to insert in your last number. As the import suffers in some measure from this inadvertency, I must solicit the favour of your making the necessary corrections subjoined.

As there appears some obscurity in that passage which commences "I observe," &c. (page 177,) I request that it may stand as follows; viz. *I observe not heat sufficient to affect our most delicate thermometers. Thus, have I suffered metallic laminæ to be fused by the electric discharge on the back of the hand, and ether &c. inflamed.*

For "two powers" read "two *powders*;" and in the same line say "rosin *or* sulphur."

In the second line succeeding, read "*or* the ball." In page 176, for "the paper as usual perforated" &c. read "the paper *was* as usual perforated, *and* a circular portion of the China ink displaced from *the* balls, *while* an indent" &c.

In the same page, dele *should* before "be framed anew" *therein,* (*with a spark taken through it, by means of a person uninsulated,*) *and surely* &c.

I must not be understood as armed with hostility against the honoured names opposed to the theory I advocate—

"Amicus Socrates, amicus Plato, sed magis amica Veritas."

If I am wrong, I shall not be ashamed to own my error, nor, I trust, be found wanting in my acknowledgements for that information which may set me right. It is by the collision of minds that truth is elicited; and this consideration should smooth those asperities which too often mingle with the sentiments of the best of men. A reciprocity or mutual interchange of opinion is not too much to hope for.

In reverting to the experiment made with the *blackened balls,* I desire

I desire to impress that it affords to *my mind* a convincing proof, that the *two fluids* (*called* positive and negative) *do not interfere with each other, on their passage in opposite directions;* and this may be further elucidated by *coating each side of the intermediate card* with *mucilage* (gum), for in this case *no bur will be raised,* and *two apertures found.*

M. Walsh, in the penultimate number of "The Annals of Philosophy," states some interesting experiments on the *electricity of paper.* The attractive power which writing-paper, being well heated, acquires by friction with caoutchouc, (and as I find, also, with *wheaten bread,* &c.) is a circumstance which most electricians are aware of. So much is this adhesive property manifested, that paper thus treated will append to a wall for some minutes. Paper is a well known *electric;—burnt paper,* according to my experiments, becomes *a conductor of electricity;* but when converted, by combustion, further, into *white ash,* it reverts to its former *electric state.*

As every fact, though humble in itself, and insulated, is necessary, before we report on the nature and properties of a new substance, I shall deviate in this instance from any thing like a natural order or arrangement.

Iode, that most singular body, a correct estimate of which seems to baffle our best researches, appears to me to be "gifted" with a most *inappropriate title,* and the same objections militate against the term *Iodine.—Iodegene* is at once expressive of its *generating a violet colour* by heat. The term *gas* most inaptly applies,—for at *common temperatures* it is *not* a *permanently elastic fluid;*—and if it should ultimately be pronounced to be *a metal,* then, according to the more modern acceptation of the nomenclature, its name will be *Iodium.*

In the external characters of *opacity* and *lustre, iode assimilates* to a *metal,* and the circumstance of its forming a *fulminating compound* with *ammonia* is allied to this opinion. The *crystals of iode* examined with *a lens* present *uniformly rhomboidal prisms. Iode* is much like *ore of irridium,* a specimen of which I have lately obtained.

I kept for some time a *glass tube* inclosing *iode* in a *tin case,* and on examining it found the tube broken, and that the *iode had disappeared,* not a *trace of it could be ascertained.* I then moistened a slip of *litmus paper* with *distilled water,* and introduced it into the tin envelope. When it was withdrawn, a few *red spots appeared on it;* and as on *contact of liquid ammonia, the original colour was restored;* there seemed evident signs of *an acid.*

When *iode* rises in *vapour,* its *odour* is somewhat *like that* of *chlorine.*

By

By repeatedly exposing this *substance* in *vapour*, and the crystalline *state, to the sunbeams*, a deposit of *a dew* of a *greenish colour* is effected; so that, though *heat* does not *alter it*, *light* produces from its *chemical affections* a most *important change*. Some of the *iode* I have now with me in a *tube hermetically sealed*, has been thus transmuted.

By raising a *perpendicular* to the *iode* in the *state of vapour*, and placing this in the sunshine, a *green shadow* is projected.

The *violet vapour* which rises on application of heat cannot be regarded as *an emanation* thrown off from the *iode*, and which *dissolves the substance*, and holds it *suspended therein*, but rather the *iode itself finely dispersed* and *comminuted*; the *alteration* of the *size of the crystalline particles*, and the *variations of the figures of the groups*, tend to the latter supposition.

Query? *Is iode the metallic radicle of chlorine?* or of muriatic gas? or a substance *sui generis, elaborated in the vegetable œconomy?*

I am, with much respect and many thanks,
Gentlemen,
Very obediently yours,
J. MURRAY.

To Messrs. Nicholson and Tilloch.

LVI. *On Alcohol or Spirituous Liquors, and on the Changes which they undergo on being rectified with alkaline, saline, earthy, and other Substances; to which is subjoined a simple Process for obtaining highly dephlegmated Spirits of Wine without Injury to its constituent Principles.* By M. DUBUE, *of Rouen**.

FOR upwards of two centuries, chemists have been proposing methods for freeing common spirits from a certain quantity of water, malic acid, and other foreign bodies which they obstinately retain, in order to procure pure alcohol, or highly rectified spirits of wine. It is now about a hundred years since Boerhaave, Cartheuser, Stahl, and other chemists were occupied with this object, and since, by means of their improvements, this valuable fluid has been obtained freed from the heterogeneous substances which alter its properties. Of all the old methods, that of Lemery seems the best: it consists in distilling in a vapour-bath spirits at 22 degrees in a matrass with a very long neck, surmounted by a head, &c.

Alcohol thus distilled marks in general from 38 to 40 degrees in the common hygrometer, (at the temperature from 5° to 12° centi-

* *Annales de Chimie*, tome lxxxvi. p. 314.

centigrade): this liquor, when at such a degree of lightness, is described by Baumé as highly rectified spirit of wine. "Its physical and chemical properties, in addition to its levity, consist in being perfectly diaphanous, very volatile, very inflammable, burning without smoke, having an agreeable smell, a hot taste, and it does not alter the aqueous tinctures of turnsole, or of the petals of violets."

Such are the characters which ought to distinguish every alcoholic extract of wine, cider, perry, rum, &c. when well rectified, deprived of water, and of the malic acid which we meet with almost always in weak spirituous liquors.

We may, by means of salt or alkaline and earthy matters, bring alcohol to a superior degree of rectification, to the point of marking 46 or more in the areometer: but it is clear that these substances act more or less on the elements of alcohol during distillation; for the liquor which results has new properties, since it acts differently with the reagents from the spirit of wine obtained without intermedium, such as that prepared by Lemery's process, which we shall henceforth call *pure alcohol*.

Lowitz, Richter, and other chemists successively suggested various substances, which have a great affinity for water, in order completely to dephlegmate alcohol. In short, if the substances mixed with spirit of wine, only take up the aqueous principle which it contained in excess before it attained its last stage of rectification, we may then conceive what immense advantages, by these processes, spirituous fluids might confer on the arts and commerce in general *; but the series of experiments about to be detailed will remove all uncertainty on this head, and enable us in future to appreciate the means hitherto employed in the rectification of alcohol. We shall show that every saline admixture changes spirit of wine more or less, either by acting on its constituent principles, or by being dissolved, or even interposed in a state of minute division.

The substances which have been alternately employed in these operations, or the *dephlegmation* of spirit of wine, are; 1st, the two fixed alkalis; 2d, the muriate of lime; 3d, the muriate of potash; 4th, quick-lime; 5th, calcined gypsum; 6th, sulphate of soda, and latterly, acetate of potash fused and reduced to powder.

In consequence I successively distilled pure spirit of wine (from

* It would be highly useful, as is now the case with most of the acids, the sulphuric in particular (the highest degree of concentration of which it is capable being known), if the *maximum* of lightness or rectification of alcohol was also fixed in an invariable manner: but in order to avoid errors, the hygrometers ought to be graduated alike in every country.

38 to 40 degrees) with these different salts, following precisely the processes pointed out.

This alcohol, when rectified with the alkali of tartar or of soda, either caustic or partly carbonated and very dry, acquires in truth three or four degrees of lightness: after this proof, its smell becomes more subtile; but it loses its natural mellowness. It greens the aqueous tincture of the petals of the violet: in addition it abundantly precipitates the water taken from wells charged with calcareous sulphate; effects which demonstrate that spirit of wine rectified over alkalis is altered in its elements, or receives in addition some heterogeneous principle.

Pure alcohol rectified over the muriates of lime and potash slightly calcined, acquires also levity, and even more than with the alkalis, but the liquor produced by it also contracts new properties. These do not belong to good alcohol: they give it a hot, bitter, pungent taste. It is easy to demonstrate the presence of the salts employed in this rectification, either by ammonia, or by the nitrates of silver and mercury, &c.

Quick-lime coarsely pulverized and mixed with pure alcohol, creates sufficient heat to permit a portion of the fluid to distil over without employing external heat: this first product gives certain signs of alkalinity, by acting sensibly on the aqueous juice of the black plum. The residue distilled in the vapour-bath contracts more and more the pungent property, and instantaneously becomes turbid on mixing with it common water saturated with carbonic acid: it is easy by putting the liquor in a large conical glass, to perceive at the bottom after it rests two days a remarkable quantity of carbonate of lime, &c.

Pure alcohol distilled over calcined gypsum also acquires lightness, less however than with the four foregoing substances: the spirit of wine produced contracts a peculiarly disagreeable smell: besides, it renders turbid the infusion of flowers of red poppy, whereas pure alcohol heightens the colour of it: it likewise communicates a shade of dead leaves to the tincture of violets,—properties which indicate its alteration, or the presence of a foreign body in the spirituous fluid.

Glauber's salts, or sulphate of soda calcined and deprived completely of its water of crystallization, seemed an excellent intermedium for dephlegmating alcohol without acting on its elementary principles. I repeated this operation several times with success, and always obtained, by employing a part of this salt pulverized over two of liquid at 36, 37 and 38 degrees, a spirit of wine marking from 38 to 40 degrees, and which had all the properties of that prepared without any intermedium: but notwithstanding its agreeable smell, it held in solution a small quantity of the saline substance employed in its distillation, for pure barytes

barytes occasioned in it a slight precipitate. It was also easy afterwards, by means of the blue vegetable colours, to discover in it the presence of the alkali set free by the subtraction of the sulphuric acid united to the ponderous earth.

By a second rectification over the same calcined salt, we may have alcohol at 42 degrees, but vain will be the attempt to give it a higher degree of levity by other distillations: this liquor differs only from spirit of wine prepared without intermedium, in so far as it contains a slight quantity of sulphate of soda in solution.

By the calcined Acetate of Potash.

This method, which was lately suggested by an eminent chemist of Paris, succeeds beyond every other: pure alcohol distilled in the vapour-bath over this salt acquires more than 46 degrees (at the temperature of $10+0$ Reaumur); but having examined this liquor, I soon perceived that it was no longer spirit of wine: it differed from it, 1st, by having a very pungent smell, something like the tincture of salt of tartar of the shops. 2d, It had an acrid soapy and bitterish taste. 3d, It changed into green the aqueous tincture of the petals of violets, &c.

It is therefore very evident that the alcohol obtained by this process is of a great levity, but the properties peculiar to this fluid remove it from the rank of pure spirit of wine, and make it a new liquid.

From the above short detail, and the facts resulting from experiments carefully made on pure alcohol from 38 to 40 degrees, extracted from the various mucoso-saccharine substances which have undergone the spirituous fermentation, it appears demonstrated that all the six saline substances above cited, have an action more or less direct, not only on the last portions of water which adhere very strongly to the alcohol, but also upon the constituent parts of this same fluid: hence we must conclude, that all these intermedia do not exactly procure a spirit of wine dephlegmated to the *maximum*, and preserving all the physical and chemical properties which ought to render it saleable and useful.

It has also been proposed to subtract the water from alcohol at 36 degrees, by distilling it in the vapour-bath with a weak dose of sulphuric acid: for instance $\frac{1}{16}$th of its weight. I have observed that so long as spirit of wine does not exceed 38 degrees, it preserves by this rectification the properties which characterize it; but if we add a new portion of acid to the latter, the product then acquires, with its specific lightness, a fragrant odour slightly ethereated: this already announces an alteration in the elementary principles of the spirituous fluid.

The common alum of commerce calcined, considering the great quantity of humidity which it loses during its exposure to fire,

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seems also very convenient for taking up the superabundant water in the composition of alcohol: in consequence I added one part in powder to two of this liquor marking 36°. After two days, the whole was distilled in the vapour-bath, and this spirit of wine then yielded 39°. It had a very agreeable taste, but I was much surprised at the new property which it had acquired: it reddened strongly the aqueous tinctures of turnsole and flowers of violet. I then rectified a third time this last alcohol over varied proportions of calcined sulphate of alumine, and obtained constantly nothing but a liquor from 39 to 40 degrees. Hence I concluded that alum deprived of its water of crystallization does not take up the phlegm which is foreign to pure alcohol, without acting directly on its constituent principles; but a weak portion of this salt is also volatilized during the operations, and held in solution, or in a state of minute division, by the spirituous fluids; for not only does it redden the blue vegetable colours, but it is also evidently disturbed by the water of barytes; effects which indicate the presence of alum in this alcohol.

I have also rectified alcohol at 39 degrees over gray calcined muriate of soda: we know that this salt retains a great part of its weight in water of crystallization, and that a red heat long continued can remove it: it was in this state that I presented it to pure spirit of wine, hoping to give it a superior degree of purity: but after several rectifications and distillations in succession of these two substances in the vapour-bath, the alcohol remained in its primitive state, and with all the properties which characterize it; which proves that the muriate of soda, even when deprived of water, does not act in any way upon the spirituous fluid at a strong degree:—only like the sulphates of soda and of alumine, a feeble portion of this salt is volatilized by the caloric and the alcohol during the operation; for this liquor becomes sensibly whiter by the addition of some drops of nitrate of mercury and silver, an effect which does not take place when the spirit of wine is very pure. It seems very astonishing that salts equally well fixed as alum is in the fire, the sulphates and the muriates of soda, should be volatilized during the distillation of the alcohol, and by a heat so moderate: but something analogous has been already noticed in No. 163 of the *Annales de Chimie*. Messrs. Dabit and Ducommun of Nantz found muriate of ammonia, carbonate and sulphate of lime, in the distilled water proceeding from a reservoir which had previously contained animal substances. Kirwan and Lavoisier also say that the nitrate of potash is *volatilized* with boiling water. I can also say that I collected very often the vapours which emanate during the preparation of the alkaline salts, or of tartar emetic, kermes mineral, sulphate of iron, acetate of lead prepared *on a very large scale,*

scale, and I found it always easy to discover traces of saline metallic substances which form the base of it; which explains in some measure the volatilization of salts which has been mentioned, and their solution or mixture in a state of minute division with alcohol after the distillation of these substances.

Reflecting on the hygrometrical properties of charcoal, and on the aptitude of pure alumine and common clay for water which it retains in abundance and even at a high degree of heat, and considering also the inertnes of these substances towards alcohol, I employed them successively in the dephlegmation of spirit of wine: consequently I made various experiments, the chief of which only I shall detail here.

1*st. By Charcoal.*

In one litre of alcohol at 36° *mean temperature,* I put four ounces 128 grammes of burnt birch wood while yet warm, shaking the whole frequently to facilitate the immersion and imbibition of the charcoal: in four days I filtered, and the alcohol still marked 36 degrees. A similar operation took place with very pure alcohol marking 36 degrees: after several days maceration over charcoal this spirit of wine had also preserved its primitive state. These effects tend to establish that charcoal absorbs both the alcohol, and the water which it contains in superabundance.

The mixtures of charcoal and of spirituous liquor were afterwards distilled to dryness in the water-bath: the alcohol, which marked originally 36 degrees, rose one degree by this operation, but the latter remained as it was. I repeated these experiments, 1st, upon animal charcoal; 2d, upon the charcoal of various woods; but I only obtained alcohol at 39 or 40 degrees even when operating on very considerable masses, and by dividing the products into fractions in order to establish *areometrical* points of contact; which proves that charcoals have not more affinity for pure water than for alcohol merely (and this has been already remarked). Alcohol, from whatever substance produced, acquires by its rectification on charcoal, a sweeter smell and a more agreeable taste than that which is obtained in the common manner or without intermedium.

By pure Alumine and common Clay.

Into one litre of alcohol at 39 degrees I put eight ounces of pure well dried alumine: after two days immersion, and always at the same temperature, I decanted with precaution a sufficient quantity of spirit of wine, and observed that it yielded 40 degrees. Afterwards I distilled in the vapour-bath to dryness. The fluid which came over had a pungent very disagreeable smell,

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and marked 41 degrees: I heated the alumine strongly in order to deprive it of about 32 grammes of moisture which it had imbibed from the alcohol, and redistilled the latter over this earth. The liquor acquired upon a second rectification a new degree of lightness, to the point of marking 42 degrees of strength *mean temperature.*

This alcohol constantly retains all the properties which characterize good spirit of wine: the smell, taste, and far less the reagents, do not discover the presence of any foreign body: its specific gravity as to that of water is nearly : : 8·292 : 10·000.

We may also obtain alcohol in its highest degree of rectification, by employing, instead of pure alumine, common potters' clay well washed, then passed through a sieve, and finally well dried before using it; but the attempt is vain in this way to give a greater degree of lightness to alcohol, as I was convinced after successive distillations and rectifications. I conclude therefore that this earthy substance has no action upon the elements of alcohol, and that it only deprives it of the water which is superabundant to its spirituous essence. In consequence of this, and as we are ignorant if this intermedium has already been employed in any similar case, we suggest this new method to chemists and distillers with confidence, and from our experiments we are of opinion that this liquor the most highly rectified, and not at all altered in its constituent principles, ought always to mark 42 degrees in Baumé's areometer *mean temperature.* We may add that alcohols of a higher degree distilled over saline intermedia are more or less altered in their constituent principles, that nevertheless the practice may be advantageous in the preparation of varnishes for the perfumer and watchmaker, and for coating metal work. But these liquors can never be called good *potable* alcohol.

We are also of opinion that alcohol rectified over saline and alkaline substances, even reduced to a proper degree, cannot be employed in the composition of medicines, because every thing inclines us to believe that it thereby acquires new properties which might deceive the medical attendant who prescribed it.

We shall conclude our present paper with some general observations upon alcohol, or ardent spirit, produced by the fermentation of every kind of mucoso-saccharine substance.

Since the fine experiments of Lavoisier, and of several chemists, we know that the inflammable liquid known by the name of spirit of wine is composed of carbon, oxygen, hydrogen, and a little azote according to M. de Saussure. Setting out from these data, we may conclude generally that all alcohols, from whatever substance produced, ought to be perfectly identical and homogeneous in their elementary principles; because, since they

are

are composed of the same radicals, the proportions of which are known, they ought to have the same properties, the same taste, and act in the same way with the reagents, being well purified, and having attained their highest degree of rectification.

This alcoholic identity may exist within the range of *physico-chemical possibilities:* but I candidly own that I was never thoroughly convinced of it; for notwithstanding numerous experiments for twenty-five years past on the alcoholic fluids produced by wine, cyder, rum, cherry brandy, fermented grain, &c. I constantly remarked that all these liquids, when distilled and rectified several times even in the vapour-bath without mixture, or the intermedium of charcoal as recommended by Lowitz, and brought to their highest degree of *spirituosity,* still exposed their origin, and that it was always easy to say which alcohol came from wine, cider, perry, rum, &c.

The odour which issues from the various kinds of alcohol when rubbed on the hands, the organ of taste, in diluting these fluids in a sufficient quantity of warm water, and their mixture with a little sulphuric acid, are the most simple as well as the most certain methods for instantly unmasking the peculiar *aroma* of each kind, and the substance from which it has been produced.

There exists a term, however, at which all these alcohols cease to be cognizable; but they are then *denaturalized,* if we may so express ourselves, and this happens in their *etherification.*

In fact, the extremely volatile odorous and expansive fluid, long known by the name of ether of *Frobenius,* which chemists prepare by distilling equal parts of alcohol at 36 or 37 degrees and concentrated sulphuric acid, may be made with every kind of spirituous liquor; and when the product which results from it is well rectified, and marks from 36 to 60 degrees in Baumé's areometer, then I do not hesitate to assert that it is impossible to ascertain to what kind of alcohol ether thus prepared belongs. It would seem that at the moment of the affusion of the acid over the spirituous fluid, the aroma which characterizes it is vaporized or destroyed by the *dissociation* or alteration undergone by the elements which compose it *.

I may add that the various alcohols at an equal degree of verification do not always produce one and the same quantity of

* By the effect of the reaction of the concentrated acid on alcohols, and by the aroma which emanates from it, we may always distinguish the kind of ardent spirit employed in this operation: but as soon as the mixture is half cooled, then the liquors which result from it are perfectly identical, as to their smell alone; but their colour is more or less intense: this seems to depend on the different proportions of the oleaginous aromatic principle which characterizes each kind of alcohol, and which is acted upon by the acid.

ether. I have met with alcohol which produced 1 1-16th more than any other. Does this arise from the heterogeneous principles which they contain more or less, or from the varied proportions of elementary substances of which they are composed, or lastly from a still stronger portion of aroma?

Without attempting to account for these varieties at present, I shall merely say that the alcohols which seem to give most ether are: 1st, that produced from perry; 2d, wine; 3d, cider. The spirits denominated cherry brandy, rum, geneva, and whisky, are far inferior to those just enumerated in respect of the quantity of ether which they yield.

LIX. *Description of a Hydro-pneumatic Blow-pipe for the Use of Chemists, Enamellers, Assayers, and Glass-blowers. By Mr.* John Tilley, *of Whitechapel**.

Sir,—Being a travelling fancy glass-blower, I work with a machine which I have contrived for my own use, and which I have been advised, by a great number of respectable gentlemen, to lay before the Society of Arts, &c. The invention consists of a tin-box, with a partition in it reaching from the top at one end to within an inch of the bottom. The vessel is air-tight at this end. It is three parts filled with water. By means of a tube reaching within half an inch of the bottom, I blow into the water at the air-tight end; the air rises in bubbles through the water to the top, and forces the water under the partition into the other compartment. The weight of the water acts upon the air which had been blown in, and forces it through a blow-pipe directed to the lamp, and keeps up a continued blast till the air is exhausted. More air may be blown in from time to time, so as to keep the blast regular and continual. It is thus I execute my fancy glass-blowing. The whole apparatus, including lamp and case, weighs only three pounds and a half.

I believe I am the first glass-blower who ever worked with such a machine.

If the Society should think me deserving of any reward, it will be very thankfully received by,

Your obedient humble servant,

John Tilley.

Direct for me at Mr. Thomas Yandall's, bookseller, Old Street Road, near Shoreditch Church.

* From *Transactions of the Society for the Encouragement of Arts, &c.* for 1813.—Fifteen guineas were voted by the Society for this communication, and one of the machines is preserved for public inspection in the Society's Repository.

The

The apparatus is applicable to the business of enamellers, jewellers, chemists, and many other arts, and can be furnished complete for 2*l.* 12*s.* 6*d.* made of tinned copper.

March 27, 1812.

To C. Taylor, M.D. Sec.

Reference to the Engraving of Mr. TILLEY'S *Hydro-pneumatic Blow-pipe.*—Plate IV. Fig. 4, 5, 6, 7, 8.

The utility of the blow-pipe, in the arts, to raise a great heat in a small object, from the flame of a lamp, is too well known to require pointing out. The assay of minerals, the arts of enamelling, jewellery, soldering metal works, but above all the blowing of small articles in glass, are purposes to which it is better adapted than almost any other mode of applying heat. The usual manner of producing a stream of air for blowing glass, is by means of a small pair of double-acting bellows, fixed beneath a table, and worked by the operator's foot; a pipe proceeds from these bellows to the top of the table, and terminates in a small jet, before which a lamp is placed, and the flame blown by the current of air upon the object to be heated. The defects of the bellows are, that the stream of air is not perfectly regular, which causes a wavering of the flame, so that it does not fall steadily upon the object which is to be heated. Mr. Tilley's blow-pipe corrects these defects, by using the pressure of a column of water to regulate the stream of air, and the supply is furnished from the mouth of the operator, by blowing through a tube, fig. 4, C, at a section of this instrument, and fig. 5 shows a perspective view of it in action. AA is a vessel of tinned iron, or copper, about seventeen inches high, five wide, and nine broad; the lid of which opens and shuts on hinges, and supports the lamp B, which burns tallow instead of oil. C is the blowing-pipe, by which the air is thrown into the vessel: this, as shown in the section fig. 4, has an inclined partition D, which divides it into two chambers, E and F; but as the partition does not reach to the bottom of the vessel, the two compartments communicate with each other underneath it: that marked F is closed at the top so as to be air-tight; but the other is only covered by the lid of the vessel, and may therefore be considered as being open to the outward air. The pipe C, fig. 4, is soldered air-tight, where it passes through the top of the chamber, and descends very near to the bottom of the vessel, deeper than the partition D does, so that its mouth is always immersed beneath the water. The metallic part of the blow-pipe G, which conveys the blast of air to the flame of the lamp, is likewise soldered into the top of the chamber F; it holds a bent glass tube, *a*, which

which terminates in a very small and delicate jet, and is fitted air-tight into the tin or copper tube G. Now, by blowing into the tube C, the air is forced out at the bottom of it, and rises in bubbles through the water into the upper part of the chamber F; this displaces a corresponding quantity of water, which passes under the partition D, into the other chamber E, elevating the surface of the column of water, and depressing the other, as shown in the figure; the water endeavouring to return to its original level, causes a constant compressure of the air, and forces it through the jet *a* into the flame of the lamp. By this means, it is not necessary to blow constantly with the mouth; for, though the air is forced into the receiver at intervals, yet the pressure of the water will expel it in a constant stream, and the operator will not be fatigued by the motion of the foot, necessary in working bellows, nor need even to keep his mouth at the pipe constantly, but merely to blow from time to time, as he finds the stream of air to decrease in its power.

The metal socket which connects the glass tube or blow-pipe *a* with the vessel A, is made conical, and the tube, having a piece of paper first wrapped round it, is bound round with cotton-wick yarn in a conical form, so as to fit the socket tight, and yet permit the tube to be moved in any required direction, to cause the air to act properly upon the flame; and the curved metal tube C is also fixed into the upper part of the tube C in the same manner. HH are the two sides of a tin frame, which is fixed in front of the vessel, and has grooves withinside of them to receive a tin plate I, which forms a screen, and can be adjusted in height so as to keep the light of the lamp from the operator's eyes, though he can see the work over the top of it: this screen is held fast by its foot being placed btween the lid of the vessel and the top of the close chamber F. K is one of two handles, which support the operator's arms while holding a glass tube or other matter in the flame, and there is another like it at the opposite side of the vessel: these handles are also wrapped round with woollen list or leather, so as to form cushions; and the vessel is steadily fixed upon a chair, bench, &c. by means of a leather strap buckled to the loops on each side of it, and passing under the chair, &c.

The lamp is made of tin, is of an elliptical, or rather of a bean or kidney shape, one side being carved inwards; across the centre of it stands a metal wick-holder, having a loop on one side of it, and which is soldered to its bottom, (see *r*, fig. 6.) Through this loop the wick of cotton is drawn, and being opened both ways, as shown in that figure, and still plainer in fig. 8, forms a passage in its middle, through which the current of air from the jet *a* passes as in figs. 4 and 8, and carries the long pointed flame upon the object to be heated. The lamp, figs. 6 and 8, is

is filled with tallow, which, melting by the heat, becomes fluid, and burns as well as oil, but with a less offensive smell, and when cold, being solid, is more conveniently carried than oil. This lamp is placed within another vessel marked B, figs. 4, 5, and 8, which supports it at a proper height, leaving a space between them all round, to receive any tallow which may run over the edge of the interior vessel or lamp.

In using this blow-pipe, the following observations being attended to, will greatly increase its effect. The long flat cotton wick of the lamp will be found to act better than the usual round cotton wick; but in either case, the flame which it raises will be considerable. The end of the glass pipe *a* must be just entered into the flame, and the current of air will throw out a cone of flame from the opposite side. If it is well managed, this cone will be distinct and well defined, and extend to a considerable length. Care must be taken, that the stream of air does not strike against any part of the wick, as it would then be disturbed, and the cone split into several parts. (A wire bent at its end, as shown at fig. 7, is very convenient to smooth the passage through the wick:) the jet of air must be delivered somewhat above the wick; and as, unless the flame was considerable, there would not be sufficient for the stream of air to act upon, for this reason the wick is opened, as shown in fig. 6, that it may expose the largest surface, and produce the greatest flame; the stream of air from the pipe should be directed through the channel or opening between the wick, so as to produce a cone the most perfect and brilliant. On examining this cone of flame, it appears to be formed of two different colours, the part nearest to the lamp being of a yellowish white, and that beyond of a blue or purple colour.

The subject which is to be heated, is held in the flame at the termination of the yellowish-white flame, where it receives the greatest heat, and is not discoloured by the soot which accompanies the white flame.

Glass tubes are, when applied to this flame, quickly rendered pliable, and may be bent or drawn out into threads or points, and hermetically sealed; or, by blowing into the other end of the tube, it may be expanded into a small globe, so as to form various small articles at the pleasure of the operator.

In chemistry, mineralogy, and the arts, the blow-pipe is an extremely useful instrument, being capable of throwing such a powerful heat on a small object, as would be difficult to obtain on a larger quantity of the same substance, in the most powerful furnaces; and with this advantage, that the process is always under the inspection of the operator; whereas he can only conjecture what passes in the centre of a furnace. In

In using the blow-pipe for experiment, a piece of charcoal is generally used to support the subject, and held in the flame of the lamp; the charcoal should be of a close compact grain, and properly burnt; for, if it is too little carbonized, it will flame like a piece of wood, and obscure the object; and if it is too much burnt, it is so quickly consumed, and burnt to ashes, that the object is in danger of being lost in it; the charcoal greatly increases the heat, by reverberating the flame, and by heating the object at the opposite side; itself being converted into fuel, and excited by the blast, and thus creates an atmosphere of flame and heated air around it, which prevents the heat being carried off so fast, or the object being so much cooled, as if it should for an instant be moved out of the cone of the flame, from the unsteadiness of the hand, or from accidental currents of air, which would disturb the flame, and cause such a wavering in the point of the cone, as to divert it in some measure from the object. In order to prevent more tallow than is necessary from being consumed, to produce the intended effect, it is convenient to have several lamps with wicks of different thicknesses, viz. one to hold two flat cottons (such as are used for the Liverpool lamps) of about $1\frac{1}{4}$ inch broad; another to hold four, and a third to hold six, or as much common wick yarn as is equal to those wicks in bulk: glass jets should also be provided of different sized apertures, to suit the greater or lesser sized wicks and flames, and deliver streams of air upon them proportionately, and their jets should point upwards in a small degree: hogs-lard is also equal or perhaps superior to tallow for the lamp.

LX. *New Outlines of Chemical Philosophy.* By Ez. Walker, *Esq. of Lynn, Norfolk.*

[Continued from p. 105.]

Sirs,—In a paper published in the Philosophical Magazine, vol. xlii. p. 161, I described an electrometer, which I had contrived for determining the mechanical forces of the two elements that compose the electric spark. From some experiments made with that instrument it appears that all electrical phænomena are produced by two distinct powers acting in contrary directions, and with equal energy. At that time I had no other way of determining the equality of those forces, than by inspection; but I have since added some improvements to the instrument, which make it more convenient and correct.

The first improvement consists in cutting a hole through the card,

card, about an inch square, between the two bobs of the pendulums*. A piece of card paper, rather less than an inch square, is inserted into the place where the card was cut out, and between the two ends of the wires, for the electric spark to pass through. As soon as this card has been perforated, it is taken out, and another piece of equal dimensions put into its place; and thus a number of experiments may be made with very little trouble.

The second improvement consists in fixing a thread to each pendulum rod. These threads pass through two separate holes in the card in contrary directions, so that by taking hold of their ends the pendulums may be drawn close to the plane of the instrument.

When an electric charge is passed through the two wires, the pendulums are thrown off in contrary directions, and consequently the threads are drawn through the card, and show the mechanical forces of the two elements.

The length of the thread drawn through the card in eleven experiments by the positive force, thermogen, was 23 inches; and the thread drawn through the card, at the same time, by the other force, improperly called negative electricity, measured 22 inches.

In four of these experiments the mechanical forces were equal: in some of the rest the positive, or thermogen, acted with greater energy than the photogen; in others it acted with less: but these differences were only such as might have been expected from the nature of the elements which were the objects of investigation.

Whence we may infer, notwithstanding this small difference, that the electric spark is composed of two elements passing through each other with *equal forces*, in contrary directions. Consequently, positive and negative are words that have no definite meanings in chemical philosophy; thermogen and photogen are more appropriate; and as I have clearly defined those terms, my meaning cannot be misunderstood by men of science.

Lynn, April 18, 1814. E. WALKER.

To Messrs. Nicholson and Tilloch.

* See vol. xlii. Plate III. fig. 3.

LXI. *Description*

LXI. *Description of a mechanical Substitute for Leeches, in Bleeding. By Mr.* J. Whitford, *of St. Bartholomew's Hospital* *.

To the Society for the Encouragement of Arts, Manufactures, and Commerce.

Gentlemen,—I have taken the liberty of offering myself a second time to your notice, and to lay before you a small apparatus I have lately invented, as a substitute for leeches: a desideratum particularly to be cherished, when it is recollected that in severe frosts leeches cannot always be procured, and very often at too great a price for the afflicted necessitous to obtain. My invention consists of a small spring instrument, with three triangular-pointed lancets, which can be regulated to any degree of depth that may be required; the rest of the apparatus is a small exhausting syringe, with three glasses, of different sizes, to be applied as the nature of the case may require; as frequent complaints have been made by medical men, who were in the habit of using the exhausting syringe and glasses for cupping, that after they had applied the glass, in the usual way, with the pump, if they withdrew the syringe from the glass for the purpose of applying others, the air would frequently get under the valve and cause the glass to fall off; and I feel happy in stating, that I have completely succeeded in the improvement I have made on the cup of the glass, which will prevent the possibility of that occurrence. Notwithstanding the many alterations I have been making on the apparatus for the last four years, I found that I was in the exact situation, as to the mode of applying the glasses, as gentlemen who were in the habit of using the exhausting syringe and glasses for cupping. I could but apply one glass at a time without the danger, if I removed the syringe, of the glass falling off, as it most frequently happens you have occasion to apply two or more leeches. I found it therefore necessary to make a further improvement on the instrument, that you might apply as many glasses at a time as you might think proper, which is done as follows:—I made a small stop-cock, on the cap of the glass, under the valve, which when the glass and syringe is used in drawing up the skin, by turning the stop-cock, makes it completely air-tight, so that it is impossible for the glasses to fall off, until you may think proper to remove them. I have the pleasure of presenting several certificates from some of the most respectable of the faculty, who have obligingly given me their opinion; and if you, gentlemen, should require further explanation respecting my

* From *Transactions of the Society for the Encouragement of Arts, &c.* for 1813.—The Society's lesser silver medal was voted for this communication.

artificial

artificial leeches, I will with pleasure attend your summons; and remain,

Your most obedient, grateful,
and very respectful servant,
J. WHITFORD.

St. Bartholomew's Hospital,
Nov. 18, 1812.

Certificates.

SIR,—I HAVE had frequent opportunities of using your leech-instrument, and am happy to inform you that it has answered far beyond my expectation. I have found it particularly useful in many cases where the indigent class of people have stood in need of leeches, but from the high price could not procure them. I have also found it very convenient in two cases, where the patients had a great aversion even to the sight of the leech, consequently the instrument proved to them its most beneficial effects.

I am, sir, yours, &c.
W. H. ELLIOTT,
Apothecary to the City Dispensary.

City Dispensary, Oct. 20, 1812.

To Mr. J. Whitford, &c. &c. &c.

SIR,—HAVING attentively examined the instrument you put into my hands, as a substitute for leeches, I am of opinion that it will afford a valuable means of supplying any deficiency of that very useful reptile, which, from its dearth and scarcity, cannot be employed in many cases.

I am, sir,
Your very obedient servant,
MICHAEL BARTLETT.

Finsbury Dispensary, St. John-Street,
Nov. 13, 1813.

To Mr. J. Whitford, &c. &c. &c.

SIR,—I HAVE made use of your artificial leech, and, from my experience, I think it will prove of great utility. In cases where leeches are necessary I think it will answer every purpose, and in many instances will be preferable. The operation can be performed much cleaner, with considerably less fatigue to the patient, and the quantity of blood taken away can be ascertained with more accuracy than if drawn by the natural leech. The extravagant price of leeches often puts it out of the power of the lower class of people to procure them; and frequently, when procured, they will be found sickly, and their power of action so languid as to disappoint the hopes of the patient, and the expectation of the practitioner; a disappointment which is prevented by the artificial leech: and where prompt and immediate bleeding by leeches

leeches is necessary, the possession of the artificial leech appears to me obvious and very desirable.

I am, &c.

Aldersgate-street, Nov. 18, 1812. J. DEARING.

To Mr. J. Whitford, &c. &c. &c.

WE are of opinion that the instrument produced by Mr. Whitford cannot supersede the use of leeches, but that it may occasionally prove a useful substitute for them.

(Signed) JOHN PEARSON,
JAMES WILSON,
THOMAS RAMSDEN,
J. C. CARPUE,
THOMAS MAINWARING,
S. COOPER,
November 24, 1812. HENRY EARLE.

I HAVE used Mr. Whitford's leech-instrument, and approve the same; I consider it a very ingenious contrivance, and that it will become an excellent substitute for leeches.

Charter-House-Square, Nov. 24, 1812. J. W. SPRY, Surgeon.

To C. Taylor, M.D. Sec.

SIR,—I HAVE great pleasure in being enabled to say, that whenever I have used your artificial leech, it has fully answered my expectations; and I cannot help thinking, that during the winter season, when leeches are so exceedingly expensive, and when generally they are more wanted, it will prove a valuable substitute. The additition of the stop-cock, as it will preserve a more *perfect vacuum*, will enhance its value.

I am, &c.

105, St. John-Street. JOHN BARNETT, Surgeon.

To Mr. Whitford, &c. &c. &c.

Reference to the Engraving of Mr. J. WHITFORD'S *Mechanical Substitute for Leeches.* Plate IV. figs. 1, 2, 3.

THIS instrument operates nearly in the same manner as the scarificator and cupping-glass, but in a more delicate way; the piercing instrument being provided with sharp points, instead of lancets, so as to make three very small punctures in the skin (instead of incisions, as by the scarificator); upon these a small glass cup is applied, and a syringe used to exhaust the air from it, and increase the flow of the blood.

Figs. 2 and 3 show the form of the instrument, called the leech;

leech; the former being a section through the middle of it, and the latter showing its outside; it is a brass tube A, having a cover B at one end, through which a screw C passes; this screw has a circular piece of brass *a* fixed upon it, which slides in the tube, and carries the three piercers *b*; a spiral spring surrounds the screw, and presses upon the piece *a*, so as to force it always towards the open end of the tube; but a milled nut *b*, upon the screw C, prevents it going too far; *d* is a small spring catch, fixed within side the tube, and having a button *e* proceeding from it, through the side of the tube; the catch passes through a small square hole in the piece *a*, and has a kind of hook formed upon it, of a similar shape to the hook of a door latch; this, when the screw C is drawn back, by pulling its nut *b*, retains it as shown in the figure, the spring being fully bent; and in this state the open end of the tube is to be applied upon the part where the bleeding is to be performed; then, by pressing upon the head of the button *e*, the spring catch *d* is discharged, and the spring throws the piece *a* forward, causing the points *b* to enter the skin, and make the punctures, the depth of which is regulated at pleasure by the nut *b*; which being screwed along the screw C, allows the points to protrude as far beyond the end of the tube as the operator's judgement directs, when the spring is discharged.

Fig. 1. is the syringe and cupping-glass, to be applied after this operation; E is a small glass bell, of which there are several of different sizes provided; it has a brass cap *f*, terminating in a screw *g*, by which it is attached to the end F of the syringe G; this exhausts the air, when the handle H is drawn out in the usual manner; a small valve of bladder being tied over the aperture in the end of the screw *g*, to prevent the re-entrance of the air when the handle of the syringe is returned; *h* is a small stop-cock, to shut up the passage when the exhaustion is complete, as the valve alone would sometimes be insufficient to prevent the leakage of the air when the syringe is unscrewed.

LXII. *Hints to Cultivators of the Sugar Cane.* By C. Blackford, *Esq. of Jamaica**.

The tenacity of cane liquor, which resisted the power of white lime to decompose and neutralise its substance, having been suggested to me, and in some instances confirmed by my own observation, it has occasioned me much painful solicitude to divine why the effect should not be the same in all cases. I was well aware, that the richer the liquor the more temper lime was necessary; the weaker juices more readily deposited its more

* From the Jamaica Royal Gazette of November 1813.

ponderous impurities, and a comparatively small quantity of white lime effected separation and precipitation. Hence, in all cases of the lighter juices, an error could not happen, and the temper-glass, when used, was a ready guide. Having lately had an opportunity of perusing a work entitled "Elements of Natural History and Chemistry," and guided by other references which it has been my study to embrace, I find there are earths and stones that may be calcareous without having the power to effervesce with acids; and my late experience has confirmed the fact, that even the best white lime, after having been exposed for some time to the air, will lose that quality which I conceive to be essential in decomposing rich, matured, good cane juice. From the frequent stoppage of fire, occasioned by the mill by no means supplying the three small vessels on this estate, the liquor often got tainted in the boilers, which its smell immediately made known. And here I must remark, that neutralised liquor assumes a new character, and, I have every reason to think, yields soon to the more powerful agent, acidity, and can only be restored by a free use of active lime applied to the boiled liquor, simply putting it into the ladle therewith, and straining or rather throwing it on a skimmer on the liquor in the coppers; nor should the operator apply lime with too cautious a hand. I take the liberty to affirm, that half a pint, or even a whole one, applied over and above in the receiver in tempering, would not have any other effect than that produced by a like quantity of salt in curing a large round of beef. So also, as a restorative, should the liquor, by staying process, lean to taint, apply liberally temper lime, to destroy the acid, and rather let the smell of alkali predominate;—do not fear the result, as no discolouring of the sugar nor scaling the tache will take place. But to come more immediately to the point: an excellent small kiln of white lime, composed of the coarse grained common limestone, and calcined by the strongest hard wood, was burnt on this estate. Previously to commencing crop this year, several hogsheads were headed up, and some was deposited in the boiling-house in casks; one burst very soon after, which for a time served the purpose of temper; but in a little I found it to lose its proper effect. My late reading had given me the idea of effervescence. On applying a half pint, or rather more, of this lime to the boiling liquor, it had not that quality, though it approached to granulation, but not in so active a degree as it ought. I caused another cask to be used, and found that with a much less quantity, thrown into a ladle of liquor, the effervescence was so strong and turbulent, as to force its way over the side of the ladle, and was more efficacious. Hence I infer, that too little attention is paid to the quality of the stone, and the state of the temper lime commonly used. From the works which

which I have had the opportunity of perusing, I am induced to believe, that, if large vessels were in use, alum would very essentially and powerfully resist fermentation. From analogy, in the animal and vegetable kingdom, I conclude, that as alum is found to resist putrefaction in a greater degree by forty times than common sea-salt, so I think it may act as a preservative from the great evil to be avoided in the manufacture of our grand staple, sugar.

Great Pond Estate, St. Ann's, June 9, 1813. C. BLACKFORD.

June 29.—Since my last communication I made the following experiment to prove how far alum acted in resisting fermentation by applying it to cane-juice in the manufacture of sugar, which I briefly subjoin:—On the 23d inst. I filled a large decanter with cane-juice as it flowed from the mill, and five three-ounce phials, prepared, the first, as a standard from the mill; into the second I put a small portion of alum; with the third I took clarified liquor from the receiver, in which there was also some alum; the fourth contained simply clarified liquor; and the fifth cane liquor out of the decanter, saturated with white lime. I then filled up the decanter with Seville orange-juice. The decanter was in a rapid fermentation the next morning. The first phial was not observed to ferment on the 24th, but on the morning of the 25th it was visible; the second, with the alum applied to it, has not to this day any sign of fermentation; the third has not yet shown any degree of agitation, though both the latter smell and taste a little acid; the fourth fermented on the 26th; and the fifth did not show any sign of fermentation until the 28th, and then slowly. Thus have I endeavoured in a crude state to ascertain the power of alum to counteract the great evil to be avoided in making sugar, leaving to those who are more able to determine with precision its due effect and properties, and humbly hoping that the attempt, however imperfect, may be considered as an offering of one using his best endeavours to promote useful knowledge.

July 7.—The phial alumed from the receiver of clarified liquor was observed to ferment on the morning of the 1st inst. The phial also alumed from the cane-juice as it flowed from the mill, began to be in a state of fermentation on the 2d inst. being from the 23d of June, both days inclusive, a period of ten days: an incontrovertible proof of the quality of alum as a powerful counteractor of fermentation. Many other grand advantages may be derived from it in the manufacture of sugar. I could make some observations that arose during the trial, but I feel I have been already too prolix.—I shall only observe, that all the phials were exposed to the same situation as to air; being also uncorked,

 and

and the others ceased to ferment some days before those last mentioned showed the least sign of agitation. The alum brightened the liquor; consequently, it would have the same effect in the sugar. This may give rise to further experiments by those more capable of elucidating its properties and effects, should you consider this as worthy of publication.

LXIII. *Experiments upon Mushrooms.* By *M.* Vauquelin*.

M. Jaume St. Hilaire, so well known by his botanical acquirements, having requested me to submit to chemical analysis three kinds of poisonous mushrooms, viz. the *agaricus theogalus*, *bulbosus*, and *muscarius*, I turned to the memoir of M. Braconnot on the subject, inserted in the lxxixth volume of the *Annales de Chimie*.

The results obtained by this chemist appeared to me to be so interesting that I immediately resolved to verify them by experiments, and with this view I began an accurate analysis of the common mushroom which is to be procured daily in the markets of Paris.

It is to the common mushroom, therefore, that my present work chiefly refers. I shall subjoin the results obtained from the three kinds of mushroom above mentioned.

M. Braconnot discovered in mushrooms a great number of principles; the most prominent of which I shall enumerate here: for instance, in the *agaricus volvaceus* there are, according to him, 1st, *Fungine*, or the fibrous part common to all mushrooms. 2d, Gelatine. 3d, Albumen. 4th, A particular kind of crystallizable sugar. 5th, Oil. 6th, Wax. 7th, Adipocire. 8th, Benzoic acid. 9th, A very evanescent deleterious principle. 10th, Finally Salts, such as phosphates, acetates and muriates of potash.

In the other species he sometimes found new acids, an unknown animal matter, and animal mucus.

ANALYSIS OF THE COMMON EATING MUSHROOM.

Agaricus campestris. After having skinned it, it was pounded in a marble mortar, adding a little distilled water, and the moisture was afterwards expressed.

These manipulations were repeated until the water which was added came off almost colourless.

The juice was filtered: it had a slight red colour, and the husks (*marc*) were set aside in order to be treated with alcohol, as afterwards described.

* *Annales de Chimie*, tome lxxxv. p. 5.

The

The husks assumed a blackish colour from the contact of the air, and the juice underwent the same change from the same cause.

The juice, when filtered and subjected to the action of some reagents, exhibited the following effects:

1. With the nitric acid, a coagulation like albumen diluted with water.
2. With acetate of lead, a very abundant precipitate.
3. With the aqueous infusion of gall nuts, the effects were the same as with diluted albumen.
4. With the nitrate of barytes, a slight precipitate.
5. With oxalic acid, no change.
6. With ammonia, no precipitate; only the red colour became brown.
7. With turnsole paper, no change.
8. This juice when exposed to heat coagulated like albumen diluted in water, and the matter which was separated from it seemed to be of a deep black.

Such are the tests to which the juice of mushrooms was exposed: they prove scarcely any thing, except that it contains a substance which acts like animal albumen, but which, according to appearances, carries with it upon coagulating a body which communicates a black colour to it.

This juice was evaporated at a very gentle heat to the consistence of a soft extract, and was afterwards treated with boiling alcohol.

Expressed Husks of Mushrooms subjected to the Action of Alcohol.

I poured upon them, when deprived of whatever was soluble in water and strongly pressed, three times their weight of alcohol at 38°, which was boiled a few minutes. The boiling liquor was passed through a cloth, and the husks were strongly pressed.

The alcohol, which had assumed a yellowish red colour, deposited upon cooling a white flaky matter, which was separated two days afterwards by filtration.

The surface of this matter when dry was of a slight brown colour.

Examination of this Substance. It was of a white colour internally, brownish externally: its consistence was solid and firm: to the touch it was unctuous: when thrown upon burning coals it melted, but imperfectly, giving out white smoke, like that of grease.

When again heated with alcohol, this matter was dissolved, excepting a small quantity of brown flakes which were doubtless the colouring principle.

It is speedily precipitated in the form of flakes, if the cooling of the solution be rapid; but when the cooling proceeds very slowly, it takes upon separating the form of crystalline laminæ, like spermaceti: it is then very white, brilliant, soft and unctuous to the touch; in short, it does not seem to differ from common spermaceti.

This substance, which was known and described by the name of *adipocire* by M. Braconnot, as one of the elements of mushrooms, exhibits a fact as new as it is interesting, since hitherto chemists have met with it in the animal kingdom only.

Examination of the Alcohol which was boiled over the Husks of Mushrooms, and which deposited upon cooling the Spermaceti just described.

This alcohol, when distilled in a retort, left a liquid residue of a brownish yellow colour, in which grumous greasy particles were swimming. This residue contained scarcely any more alcohol; it was the water contained in the mushroom husks, at the moment of being treated by the alcohol, which gave it fluidity. This liquor was filtered; and after having separated the fat matter from it, it was evaporated into a syrupy consistence.

Examination of the above greasy Matter.

This substance is of a brownish red colour: it is of a concrete but soft consistence: it is melted and reduced by heat into smoke, which has the smell of common fat: the taste is not caustic, but it has something sharp. It is soluble in alcohol, particularly by means of heat.

If it was this substance which M. Braconnot took for wax, I think he was mistaken: it is rather a kind of fat, which it resembles more than wax; but it is probably not this body which M. Braconnot described by the name of wax.

Examination of the fibrous Part of the Mushroom, called Fungine *by M. Braconnot.*

The mushroom having been cleansed, as observed above, successively by water and by alcohol, from every thing which was soluble in these two agents, it was afterwards dried, and submitted to distillation.

The following are the products which it gave:

1. A brownish liquid which reddened strongly turnsole paper, but which contained however ammonia, which was detected by potash.

2. A thick brown oil which had the smell of tobacco smoke.

3. A yellow substance under the form of crystals attached to the

the neck of the retort, and the nature of which could not be ascertained.

4. A charcoal, the parts of which were not joined into one mass by heat.

5. This charcoal, when burnt in a platina crucible, gave a small quantity of whitish ashes, mostly composed of phosphate of lime and lime itself.

This result differs essentially from that which M. Braconnot says he obtained from *fungine* subjected to the same operation, since it announces that its product was ammoniacal, without however containing carbonate; which appeared to be impossible.

I am doubtful whether we ought to form a particular principle of this substance: its numerous analogies with the common ligneous principle seem to indicate that it is merely this principle slightly modified by some matter which remains mixed or combined with it.

Juice of Mushrooms extracted by means of Water.

It will be recollected that the juice of mushrooms so obtained was evaporated to the consistence of a soft extract, and afterwards treated with hot alcohol.

The latter dissolved a considerable quantity of matter, which communicated to it a brownish red colour; but it also left a certain quantity of other substances, the greater part of which consisted of albumen coagulated during the evaporation of the juice.

The first portion of alcohol not appearing to act any longer, it was decanted, and replaced by a second, and so on until this fluid was no longer coloured.

All the portions of alcohol were collected, and distilled in the vapour bath, in order to obtain the principles which were dissolved in it. We shall recur presently to this part of the operation; in the mean time we shall examine the matter insoluble in alcohol.

I began by washing it in water, which extracted a deep brown colour from it: when fresh quantities of water, passed over this matter, no longer took any colour, it was dried by a gentle heat, and a portion was distilled. The washings above mentioned were evaporated, and put by themselves to be used as will afterwards appear.

When once dried, this matter was of a blackish colour, and was considerably hard and brittle: the fracture was shining: when put upon burning coals it softened, and emitted a smoke the smell of which resembled burnt horn.

When distilled, it furnished abundance of carbonate of ammonia crystallized in the neck of the retort: another portion

in the liquid state, abundance of reddish and thick fetid oil; lastly, a charcoal which joined in a single lump, although the substance had been reduced in very small fragments.

From these properties, it does not appear doubtful that the substance in question is of an animal nature, and of the albuminous kind. This did not escape the sagacity of M. Braconnot.

The presence of albumen in these vegetables explains the reason of their assuming, when roasted, a consistency and firmness which they had not when raw, being brittle and very spongy.

It also accounts for their soon becoming putrid, and emitting a fetid smell, when they attain their highest point of vegetation; and also shows that they afford substantial food for carnivorous animals, whereas herbivorous animals never touch them. We shall see, however, that albumen is not the only animal substance which mushrooms contain.

Examination of the Matters dissolved by Alcohol when applied to the Extract of Mushrooms.

We have already observed, that the juice of mushrooms evaporated to a soft extract had been treated with alcohol, which had dissolved that part of it which had been obtained separate by the evaporation of the solvent.

This reddish-brown substance, when dried by a gentle heat, was again treated with alcohol: it was divided into two portions: the most abundant was dissolved by alcohol, and another portion remained at the bottom in the form of a brown extract. We shall see that this last resembles that which remained with the albumen, and which we separated from it by means of water.

The alcohol filtered warm deposited upon cooling a crystalline substance in the form of silky white needles, and having a taste slightly saccharine. On reducing by evaporation the quantity of alcohol, we obtained a fresh quantity of this substance.

Examination of the above crystalline saccharine Substance.

This substance, after having been separated from alcohol and dried on Joseph paper, was of a yellowish white colour: it was dry and brittle. When dissolved again in boiling alcohol, it reappeared of a perfect white colour, and always in the form of fine and silky needles.

It is by no means so sweet as common sugar: the smell which it gives out when burnt does not resemble that of the latter: it is sharper, and has more analogy to the smoke of wood. Concentrated sulphuric acid dissolves this substance, and makes it take a red colour; but it does not char it as common sugar does. If we put water into a mixture of this sugar and of sulphuric

phuric acid, a white coagulum is formed, which upon agitation renders the liquor milky.

Although this saccharine principle be more soluble in water than in alcohol, it is nevertheless not more so than common sugar, and it crystallizes much more easily than the latter.

I was at first inclined to think that this kind of sugar was similar to that which M. Fourcroy and I discovered in the *algæ*, melon, onion, and manna: but as M. Braconnot assures us that the sugar of the above plants ferments, whereas that of mushrooms does not, I forbear giving an opinion until I obtain a quantity large enough to verify the fact. In the mean time I regard the sugar of mushrooms as being *sui generis*.

Examination of the extractive Matter of the mushroom which was insoluble in Alcohol.

We have already shown that, by treating the extract with alcohol, the former was divided into two portions, one of which was dissolved, and the other remained at the bottom: it is of the latter that we shall now speak.

It is of a deep brown colour, and has a decided and rather pleasant taste of mushrooms. Its solution in water is abundantly precipitated by the infusion of gall-nuts, as well as by the nitrate of silver: when evaporated into a syrupy consistence, it furnishes crystals of a pungent taste, but entangled in the extractive matter.

As it appeared to be impossible to separate these crystals so as to have them pure, a portion of the matter which contained them was burnt in a platina crucible.

The following phænomena were then exhibited: 1st, A bubbling took place on account of the humidity which still remained, and a very agreeable smell of mushrooms was emitted. 2d, Afterwards ammonia was sent out in such quantities, that it affected the eyes and nostrils, and instantly revived the colour in turnsole paper which had been reddened by an acid and moistened. 3d, It furnished a charcoal easily incinerated, and which left an abundant grayish ash, the taste of which was salt and pungent. 4th, These ashes were almost entirely dissolved in water, there remaining only some slight molecules of charcoal. The reagents indicated that the salts contained in the lixivium of these ashes were the phosphate, the muriate, and the carbonate of potash.

If it be asked, what is the nature of this substance? does it belong to a species already known in the organic kingdoms? or is it a peculiar and new composition? I can scarcely answer the questions in a satisfactory manner.

M. Braconnot found, it is true, in mushrooms, gelatine and animal mucus; but when I compare this substance with either of the above, I find no perfect identity. In fact, gelatine properly evaporated

evaporated passes into a jelly more or less solid upon cooling, which the substance in question does not: the gelatine is not dissolved in alcohol even weakened to 24 degrees, and the latter is dissolved in alcohol at upwards of 30°.

On the other hand, animal mucus, on being united with water, communicates to it a consistence and a viscosity of which the latter is not susceptible.

Besides, animal mucus is not sensibly precipitated by the aqueous infusion of gall-nuts, whereas the substance in question is abundantly.

Can there be a portion of albumen, which in consequence of the long continued action of heat has become soluble in this fluid? I am inclined to think so; but I shall not assert it positively until I make some comparative experiments on a future occasion.

Examination of the Principle of the Mushroom which is soluble in Water and in Alcohol.

This substance is of a reddish brown colour: in taste and smell it resembles broth a little salt.

When dried by a moderate heat, it preserves its transparency and becomes brittle: in this state it keenly attracts humidity from the atmosphere, and all its parts become glued together on the application of moisture.

When thrown upon burning coals it melts, and is reduced into smoke which has the smell of fried meat mixed with fat. It furnishes upon distillation a thick brown oil, and carbonate of ammonia: its voluminous charcoal is presented in a single piece.

There were found in the charcoal of this substance muriate of potash and subcarbonate of potash, proceeding no doubt from the decomposed acetate.

The solution of this substance in water is entirely precipitated by the infusion of gall-nuts.

When triturated with a little potash and water, ammonia was liberated in a very marked manner: it contains in fact a little muriate of ammonia.

From the above details it appears that the substance in question has all the properties at present known of *osmazome*, and as such in my opinion it may be considered. I found it in all the mushrooms I examined.

Eight of the various substances found in the common mushroom are produced by vegetation: viz. 1. Adipocire. 2. Oil or grease. 3. Albumen. 4. Saccharine matter. 5. The animal substance soluble in alcohol or water (*osmazome*). 6. The animal substance insoluble in alcohol. 7. The fungine of M. Braconnot,

Braconnot, or fibrous part of the mushroom. 8. The acetate of potash.

It is extremely remarkable that the mushroom, whose structure is so simple, so homogeneous and short-lived, and which seems to avoid the light, should form so great a number of different principles, and in such considerable quantity.

Agaricus bulbosus. It contains: 1. The animal matter insoluble in alcohol, similar in every respect to that of the common mushroom. 2. The animal matter which is soluble in alcohol and water, which I take to be osmazome. 3. A fatty substance of a yellow colour and an acrid taste. 4. An acid salt which is not a phosphate, for it does not disturb lime water. The husks of this mushroom furnished an acid product by distillation.

Agaricus theogalus. It contains: 1. The crystalline saccharine matter. 2. The greasy and bitter matter. 3. The animal matter insoluble in alcohol. 4. Osmazome. 5. An acid vegetable salt.

Agaricus muscarius. It contains: 1. The two animal matters above mentioned. 2. The fatty matter. 3. Muriate, phosphate, and sulphate of potash.

The parenchyma of the two latter *agarici* gave also an acid product upon distillation.

The above three kinds of mushrooms probably contain other principles; but the small quantity which I possessed did not afford me an opportunity of entering upon a detailed examination. I shall take up the subject on a future occasion: in the mean time, if they contain any deleterious substance, it must be looked for, in my opinion, in the fatty matter already described.

LXIV. *Analysis of a new Variety of Ore of Antimony. By M.* Klaproth *.

The subject of this analysis is a new mineral, which has been recently found in the county of Sain, at Treusbourg. My preliminary experiments gave me as its chief constituent parts nickel and antimony. The novelty of this union induced me to undertake a rigorous analysis.

Its colour is gray inclining to iron black: it is compact, of a middling metallic lustre, lamellous fracture, and indeterminate fragments:

* From *Magazin der Gesellschaft Naturforschender-Freunde.* Berlin, Jan. 1813.

fragments: it is easily pulverized. The specific gravity of a specimen freed by washing of a brown ochre proceeding from efflorescent sparry iron is 6·580.

A. *a.*) 300 grains of ore, purified from iron as much as possible, were brought in contact with a mixture of five parts of muriatic acid and one of nitric acid. When cold, the solution of the metals had already taken place in a great measure, and the sulphur was separated. This residue, when treated again with the same acid cold, mixed, and washed afterwards with alcohol, gave 51 grains of dry sulphur. When burnt there remained ten grains of a blackish residue, which was almost wholly dissolved in the above mixed acid by means of a slight heat.

b.) The solution was concentrated in a retort, and the liquor when highly concentrated was diluted with water, which occasioned an abundant white precipitate.

The liquid decanted from the precipitate was concentrated again, decomposed afterwards by water, and the new sediment added to the former. This precipitate, which at a cursory glance might have been confounded with Algarotti's muriate of antimony, was recognised to be an arseniate of antimony.

c.) After having separated the precipitate from the solution, it was again concentrated, and appeared then to be of a deep green. Ammonia in excess was poured into it, which redissolved the precipitate except the brown oxide of iron, the weight of which was found, after washing and calcination, to be thirteen grains and a half.

d.) The ammoniacal solution was evaporated to dryness, and the saline mass was afterwards made slightly red-hot in a porcelain crucible, in order to decompose the neutral ammoniacal salt. The residue of muriated oxide of nickel, which presented a heap of shining micaceous laminæ of a brownish yellow, was wholly dissolved in the muriatic acid by means of heat, yielding a solution of a grass green colour. By means of caustic potash, the oxide of nickel was precipitated from it, which in the state of hydrate was of an apple-green colour. This precipitate when well washed, dried, and made red-hot in a platina crucible, left $93\frac{3}{4}$ grains of pure oxide of a schistous gray colour. In order to determine the relation of this oxide to metallic nickel, we dissolved in nitric acid 100 parts of pure nickel obtained by reduction of the *chrysoprase* and the *pimelite*, and the solution was precipitated by means of potash. The precipitate when washed, and made red-hot in a platina crucible, yielded 132 and a half of oxide of nickel: according to this, the above $93\frac{3}{4}$ grains of oxide of nickel yielded $70\frac{3}{4}$ grains of metallic nickel.

B. The precipitate *b.*) composed of arseniated oxide of antimony

mony was subjected to several experiments with a view to determine the proportion of arsenic, but the results were not successful. The object was obtained, however, in a satisfactory manner by the following process.

a.) 200 grains of ore pulverized and mixed with 600 grains of nitrate of potash were gradually introduced into a red-hot porcelain crucible: this mixture occasioned but a slight detonation. The pasty mass of a clear brown was diluted with warm water, and sufficiently washed in this menstruum. We added to the alkaline liquor a slight excess of nitric acid, of which a small quantity only was requisite for saturation. The liquor was not made turbid by the addition of the acid: a proof that the potash had dissolved none of the antimony. I poured in lime water, which occasioned a voluminous precipitate. This precipitate when washed and dried was mixed with one-third its weight of charcoal, and sublimed in a small retort. The metallic arsenic sublimed in a shining crystalline stratum weighed 19½ grains. The residue in the retort mixed with one-half its weight of boracic acid, and heated again, sublimed two grains and a half more of metallic arsenic.

b.) The brownish substance when well washed was digested with a mixture of seven parts of muriatic and one of nitric acid: almost complete solution was the consequence. The antimony was precipitated from the filtered liquor by water.

The precipitate when well dried weighed 116 grains. As in a comparative experiment 100 parts of metallic antimony gave 130 of white oxide, the 116 grains represent 89 grains of metallic antimony.

C. In order to determine with equal accuracy the quantity of sulphur in this ore, 100 grains were distilled with nitric acid, and this operation was repeated with additional portions of acid until all the sulphur had disappeared from the solid residue. The matter remaining being well washed by water, and the liquor mixed with muriate of barytes, gave 102 grains of sulphate of barytes, in which the sulphuric acid answers to 14½ grains of sulphur.

The constituent parts of this ore will therefore be, saving a few fractions,

Nickel-metal	A *d*)	23·50
Antimony-metal	B *b*)	44·50
Arsenic-metal	B *a*)	11·
Sulphur	C)	14·25
Oxide of iron	A *c*)	4·50
		97·75

But

But as the oxide of iron seems to be owing to the matrix of the ore only, we may fairly adopt the following results:

Nickel	25·25
Antimony	47·75
Arsenic	11·75
Sulphur	15·25
Total	100·

LXV. *Process for making a useful Paste from Potatoes, for the Use of Weavers, Bookbinders, Trunkmakers, Upholsterers, &c. By Mr.* CHARLES DRURY, *of Mansfield, Nottinghamshire*.*

SIR,—PERMIT me to lay before the Society of Arts, &c. a substitute for wheat-flour paste. I have given it a fair trial for ten months past, in order to convince myself and others of its usefulness. From these experiments, I have no doubt it will prove equal to, if not surpass, that made from flour, for paper-hangers, stationers, weavers, trunkmakers, calico-printers, &c. and also for labels, cards, and pasteboard, as it may, with ease, be made free from lumps, and does not admit the air to get under the paper as in common paste, or injure the colour. It is free from any offensive smell, and not above one-third of the price of that prepared from flour. The use of potatoe paste will cause a saving of many thousand bags of flour annually, to be applied to the nourishment of mankind.

One peck of potatoes, when prepared, will make upwards of thirty-eight pounds of paste, and one acre of land planted with potatoes would produce more than many acres of wheat. I shall be happy to attend, and give the Committee full explanation upon the subject.

I remain, sir,
Your most obedient, humble servant,
CHARLES DRURY.

Mansfield, May 1, 1812.
To C. Taylor, M.D. Sec.

Process for preparing Mr. DRURY'S *Paste from Potatoes.*

Take one pound of raw potatoes well washed from dirt, grate them fine on a common tin grater, without paring them, into two pints and a half of water; then boil the whole mixture imme-

* From *Transactions of the Society for the Encouragement of Arts, &c.* for 1813.—Ten guineas were voted for this communication.

diately,

diately, and stir it well during the whole time of boiling, which should be about two minutes; then remove it from the fire, and add to it half an ounce of finely-powdered alum, by gradually sprinkling it into the paste, and stirring it with a spoon till the whole is perfectly incorporated. It will then be fit for use, and forms a beautiful transparent paste.

Letter from Sir John Sinclair, *Bart. President of the Board of Agriculture.*

Dear Sir,—This will be delivered to you by Mr. Charles Drury, whose mode of making paste seems entitled to the attention of the Society of Arts, &c.

I remain, dear sir,
Your very obedient servant,
John Sinclair.

27, Old Burlington Street,
May 29, 1812.

To C. Taylor, M.D. Sec.

Certificates were received from Mr. Richard Free, trunkmaker, 115, High Holborn; Mess. J. Viney and Co. trunkmakers, 122, Aldersgate Street; and Mr. F. Clark, bookseller, 33, Piccadilly; all stating Mr. Drury's potatoe paste to be equal to that made with flour; and that, after being made ten or twelve days, and exposed to the air, it did not appear to be in the least changed.

LXVI. *Description of a splendid Meteor seen at Dublin on the 17th of April* 1814.

Sirs,—As I believe that you will not be unwilling to insert in your journal any information which may have for its object the advancement of science, I shall describe to you, as well as I am able, a most remarkable meteoric phænomenon, which extended across our horizon from about north north-east to south-west on Sunday night, the 17th of April, and remained so for a considerable time.

About nine o'clock P. M. I observed the horizon towards the west north-west strongly illuminated. I at first imagined that this appearance was caused by the zodiacal light; but as the sun descended, the light extended itself, until at length it covered nearly the whole hemisphere, and occasionally almost reached the zenith. About midnight the light was so strong that the city was illuminated as if there had been moon-light. At the window where I sat observing it I could see to read. At one o'clock it still continued to diffuse the same splendour.

It

It was by some circumstances connected with this phænomenon that my attention was attracted, much more than by the brilliant light which it diffused; a portion of the sky towards the north-west appeared at first very dark, as if covered by a heavy cloud. When I took notice of it, it rose about 15 degrees above the horizon, and appeared as if it was a section of an arch of considerable extent. It gradually increased, preserving the same shape, until it attained an elevation of about 45 degrees, and covered nearly one half of the horizon. I should have supposed it to have been a very dense cloud, had the stars not been visible through it. That beautiful star Capella appeared to sparkle through it with increased brilliancy. The circumference of this immense body of darkness was covered all round with a strong yellowish light, resembing the morning's dawn when the sun approaches the horizon: this circle of light was in depth about five degrees, in some parts it was broader, in others narrower.

The wind was at south. The sky was sprinkled with heavy unconnected masses of clouds, of the kind called *cumulus*: by the direction of the wind, they approached towards the north. According as each cloud arrived at the dark space above described, it rapidly diminished in size, until it was entirely dissipated. It appeared to me as if the gradual increase of size of the dark space was owing to the quantity of heavy clouds which were decomposed when they came into apparent contact with it.

When the dark space had attained its greatest size, I observed a gleam of light shooting across it, in two or three places, close to the horizon, and immediately the upper part of it all round began to move. The brilliant circumference was tinctured in many places with prismatic colours, and appeared to be composed of bundles of radii emanating from a centre. Flashes of light the most vivid darted from every part of this vast circle, reaching to the zenith; the whole mass was in motion, and presented a more sublime and splendid appearance than can well be imagined.

By degrees the dark space diminished; the coruscations became fewer and less brilliant, and the sky resumed the same appearance as it had at first: but it did not long continue so—the clouds again moved in the same direction—the phænomena were repeated, and the same grand spectacle was again exhibited. As I ceased to observe these appearances at one o'clock, I cannot tell how long they might have continued.

The aurora borealis has not, I believe, been observed for some years in these latitudes. I remember having seen it, at a time when I was not accustomed to pay much attention to such matters.

matters. I am not sure whether the phænomena I have been describing, may not have been connected with its reappearance with increased brilliancy. I do not know whether there had been observed any connection between the aurora borealis and the decomposition of clouds, as appeared in this instance.

As this singularly beautiful phænomenon must have been seen by many thousands beside myself, I should be glad to hear an account of it from some intelligent observers.

Yours, &c. OBSERVATOR.

P.S. On the morning of Sunday there was a good deal of rain; there was also some rain the two preceding days. For nearly two months before, very little rain had fallen.

18th April.

To Messrs. Nicholson and Tilloch.

LXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

March 24. DR. YOUNG's Remarks on the Employment of Oblique Riders, and on other Alterations in the Construction of Ships, were read.

This paper contains a theoretical discussion of the mechanical properties of the different arrangements of timbers in ships and other fabrics, according to their direction and modes of combination. The author first investigates the nature and magnitude of the forces to be resisted, which are the weight, the pressure of the water, the impulse of the wind, and the resistance of the ground, or of a rock: and the action of the pressure in a longitudinal direction, as a cause of arching, which has not hitherto been observed, is particularly examined. The force of the waves, both in a vertical and a transverse direction, is estimated as far exceeding in magnitude the more permanent causes of arching, although from its transitory nature it has been less commonly noticed as requiring particular counteraction. He next adverts to the insufficiency of a hasty view of the subject for the determination of the best arrangement, and shows that it is the stiffness of a fabric, and not its ultimate strength, that is most materially affected by the obliquity of arrangement, where the depth is given, and the fastenings are perfect; and that the question of the utility of oblique braces must be determined by the manner in which a ship usually breaks, whether by the alteration of the angular situation of the parts, or by the giving way of the fastenings. From actual observation he concludes that about half of the effect is generally produced in one way and half in the other; and hence infers that the advantage of oblique timbers must be very considerable, provided they be sufficiently

strong to resist the forces to which they are peculiarly exposed: and he finds upon ,calculation that Mr. Uppings's braces are likely to be strong enough to resist any force which can easily be applied to them. He concludes by recommending the cautious adoption of these improvements, the most decided of which appears to be the filling up between the timbers in the hold, as both increasing the strength, and materially tending to prevent the decay: and even the obliquity of the planks of the decks, he thinks, may in some cases be of advantage, especially when it becomes neccessary to have the ship hove down on her side.

March 31. A paper by S. Groombridge, Esq. was read on Atmospherical Refraction, correcting Observations on the circumpolar Stars by the barometric Pressure and thermometrical Temperature.

This may be considered as a supplement to the author's former paper on atmospherical Refraction; wherein he had proposed a certain formula for the computation of the quantity, from observations made to less than 80° of zenith distance. Having since applied this formula from 80° to 89° 42′, he has found that the refraction so computed exceeds that observed from 80° to 88°; and below 88° gives the quantity less than from observation. He now proposes another formula, which will agree with the observed quantity to 87°, and, corrected in a certain ratio, thence to the horizon.

The computation of the refraction, with the corrections for the barometer and thermometer, from the tables prescribed by the author, will be very direct and simple, and obtained without the tedious process of logarithms. In the result they will agree nearly with the French tables, to within 2° of the horizon; when both the observation and the refraction become uncertain.

Part of a paper by Dr. Olay was read, on that part of perspective which relates to circles as distinct from conic sections. The author acknowledged his obligations to Mr. Kerrick, the librarian of Cambridge University; stated candidly what was original, and what was only digested from others; and laid down six propositions, only a part of which were of a nature to be read. The Society then adjourned, in consequence of the holidays, till

April 21. The Right Hon. President in the chair. The first part of a letter from Dr. Brewster to the President was read, detailing some further experiments and curious discoveries on light and colours, particularly some optical effects of mother of pearl. The prismatic colours exhibited by mother of pearl have always been considered as furnishing a good demonstration of the Newtonian theory of colours, by its diversified laminæ; and Dr. B's experiments do not contradict this explanation. He found that mother of pearl polarizes light as well as agate and other bodies, in addition to which it has the singular property of communicating

municating its power of developing the prismatic colours to resin, wax, cement, gold-leaf, lead, &c. and this communicated quality is retained even after exposing the body to a considerable degree of heat, so that it is not actually fused. This fact was discovered by Dr. B. in consequence of having cemented a piece of polished mother of pearl on a goniometer to take the angles of refraction. He cut and polished the mother of pearl as well as possible in various modes, but still found it retain its striæ or cavities resembling the fine lines used by map engravers to define the limits of land or water. To these striæ he attributed in some degree the formation of the prismatic colours.

IMPERIAL INSTITUTE OF FRANCE, FOR THE YEAR 1813.

[Continued from p. 232.]

Mathematical and Physical Sciences.

The parabola itself is only a limit, a mere single case between the ellipse and hyperbola, the dimensions of which may vary *ad infinitum*. Hence there is scarcely any probability of a parabolic orbit. The hyperbolas are scarcely perceptible, and from experience it would seem that the ellipses are hardly more so. Hyperbolic comets never return—elliptical return after a long interval. There is no reason for being surprised that only one has hitherto returned constantly.

Laplace has discovered in the attraction of Jupiter a probable cause why the comet of 1770 has not appeared eight times since that period. Perhaps it is entirely evaporated, or reduced to a nucleus so small, and so little luminous, that it will remain for ever invisible. Both these explanations are not incompatible; and they may have concurred in producing an effect which has excited the curiosity of mathematicians and astronomers.

It may be presumed that this new hypothesis will considerably diminish the importance of comets: it is similar to the opinion of the ancients, who considered comets as temporary collections of vapour which are speedily dissipated and destroyed. Hence the astronomers of old did not think them worthy of being scrupulously observed. All the difference is, that Aristotle places comets below the moon; whereas, according to Dr. Herschel, they are formed beyond the planetary system, and last much longer, since it is only in their perihelion that they are liable to lose a part of their substance. But if they become invisible, it is the same thing to us as if they did not exist.

M. Burckhardt's Memoir on the Quantity of Matter in the Planets.

The formulas of MM. Lagrange and Laplace enable us to assign

assign the situation and dimensions of the planetary orbits for any epoch. In a mathematical sense, the problem is resolved. The arbitrary constant quantities are mostly determined with a sufficient degree of accuracy for these researches. We are acquainted even with the quantity of matter in those planets which have satellites, as Uranus, Saturn, Jupiter, and the Earth; but Mars, Venus, and Mercury have no satellites. We have no other means of determining the quantity of matter in them than the alterations which they produce in the eccentricities and inclinations, or the equations which they give for the movements of the aphelions and nodes. But these variations are extremely slow. Good observations go no further back than 60 years. There remain only the periodic equations of the longitude, and these equations are not greater; but their periods are shorter. Half a period is sufficient to obtain a double effect, since it is alternately positive and negative. The moon is nearly in the same situation with Venus. Notwithstanding the assistance drawn from the tides, &c. we have not as yet an exact knowledge of the quantity of matter in our system.

Yet unless we adopt at least a hypothetic value for these unknown quantities, it is impossible to have exact tables of the apparent motion of the sun. Fortunately, during the last sixty years we have a prodigious number of good observations. The estimates of the quantity of matter in these planets, which agree best with the average of these observations, will be, if not precise, at least the most probable values of these doubtful quantities.

In order to determine them, the author of the tables of the sun had chosen, out of all the observations which he had calculated, those where each of the quantities of matter in the planets produced sensible effects. The results which he obtained did not appear even in his own eyes so certain, that they might not be somewhat changed, either from other observations, or from the same observations differently combined, especially if different elements be used in reducing them, such as the right ascensions of the stars.

It is the same with the mean secular motion of the sun. He had determined it by the comparison of a great number of observations made about 1752 and 1800, which only gave him the movement of 48 years, that is, a little less than one half of the secular motion. He presented this movement not as certain, but as agreeing best with the observations calculated by himself. He perceived, that the slightest change in the position of the stars at the two extreme epochs would introduce an equal change in the movement obtained. He did not venture to affirm that this movement was preferable to that which M. de Zach proposed

posed about the same time; and which was fixed upon by himself in his first researches. He remarked in his preface, that *time alone could decide upon a point so delicate.*

But whatever is to be obtained in a long interval of time, and by more numerous and more precise observations, may likewise be obtained, at least in a certain degree, by redoubling present vigilance, multiplying calculations, and employing more correct data. This is precisely what M. Burckhardt has attempted.

He began by calculating 36 years of observations from 1774 to 1810, in order without inconvenience to be able to dispense with the small equation of the latitude of the sun, which ought to pass several times over all the values of which it is susceptible, in this double revolution of the nodes of the moon. He employed besides 310 observations of Bradley in 1752. By this means he gained eight years, which have elapsed since the construction of the last tables. To calculate these observations, he took a mean between the corrected right ascensions of Maskelyne and those of M. Bessel. The author of the tables had employed the right ascensions of Maskelyne corrected by his own observations in 1800. And for 1752 he had taken the right ascensions of Bradley, newly corrected by Hornsby, Bradley's editor. From these changes, which the researches and observations made of late years rendered possible, there ought to result a difference in the mean motion, and probably likewise in the value of the quantity of matter in the planets.

M. Burckhardt finds 3·8″ to be added to the motion for 49 years, which gives 7·7″ for the secular motion. This motion is the mean between that of Zach and that published by La Caille about 56 years ago. Mayer, who attempted to correct this motion, considerably increased the small error in it. Lalande diminished Mayer's motion 20″, and gave a quantity 8″ greater than that now given by Burckhardt.

We see at least that in these oscillations the error is always diminishing, and that, if we have not yet obtained the true quantity, we have made an approach to it. The mean length of the year, according to M. Burckhardt, is 365 days five hours 48′ 49·7″. The author of the tables made it 51·5″. But in the second volume of his Astronomy, which was published about a year ago, we find that he inclines to 50″, and was therefore himself approaching to the new determination. The difference is now only 5·3″, a quantity respecting which it will be always difficult to decide.

The author of the tables had found for the lunar equation 7·5″; La Caille supposed 7·05″, Maskelyne 7·1″, M. Burckhardt 6·8″. The mean will be 0·15″. So that the uncertainty is reduced to a small fraction of a second.

 M. Burck-

M. Burckhardt finds that the quantity of matter in Mars previously determined must be dimished $\frac{1}{20}$. Now as the equation itself is very small, this correction, very uncertain on many accounts, does not deserve any attention.

The most considerable correction is that for Venus; and as the equation is much more sensible, the uncertainty ought to be much less. M. Burckhardt diminishes its quantity of matter $\frac{1}{9}$, which will produce a diminution of about 1″ in the greatest equation.

The quantity of matter was supposed by Laplace	1·0000
The author of the tables made it	1·0743
M. Burckhardt reduces it to	0·9549
M. Lindenau has lately found it	1·0797
and	1·1131

The mean of these four results is 1·0555, and differs only $\frac{1}{50}$ from that supposed in the tables.

It was by the movements of Mercury that M. Lindenau endeavoured to determine the quantity of matter in Venus. He has united the results which he obtained from the motions of the aphelion and the nodes. His mean is 1·0964, so that he augments the equation of the tables as much as we should diminish them if we were to make choice of the above mean. Between these opposite testimonies, the author of the tables may adhere to his own number. But he puts no greater confidence in his determination than in any other. He even admits that the result obtained by M. Burckhardt, founded upon a greater number of observations, and upon newer researches, offers in consequence a greater degree of probability. A reason of great weight adds strength to the necessity of diminishing the mass of matter of Venus, and this reason long excited distrust in the author of the tables. In whatever manner he combined the observations of Lacaille, Mayer, Bradley, Le Gentil, Maskelyne, Piazzi, and his own, he could never obtain more than 48″ for the secular variation of the obliquity of the ecliptic. The mean obliquity which he found in 1800 has been since confirmed by all the solstices observed at Paris. That which results for 1750, from so many observations agreeing remarkably well with each other, cannot be wrong further than 1″. Hence he concludes that the secular diminution cannot be greater than 50″. He has never believed that it amounted to 52″. We may therefore ascribe to Venus a quantity of matter which would give 48″ or 50″ for the secular diminution, and give to the equation of Venus in the solar tables the value which will result from this supposition.

M. Burckhardt proposes a diminution of 1″ for the greatest equation of the centre. If we collect together all these corrections, it will follow, that the sun's place, calculated at present for 1850,

1850, may differ 6″ from a perfect observation made at that time. But, before this can happen, we must suppose, what is scarcely possible, that all the three errors are at the maximum, and have the same sign. The greatest error that can be supposed is 3″ at the end of 50 years. We wish the instruments by that time may be brought to such perfection that an astronomer can make a single observation with no greater error. But this slight error may be easily avoided by adopting the corrections of M. Burckhardt. The most important of them is that of the secular motion. It is easy to diminish it $\frac{1}{10}$ of a second per year. The two other corrections would give still less trouble; but as they are periodic, and often of no consequence, they may in many cases be neglected.

Corrections so little sensible as not to pass the limits of the errors of the best observations, may pass for a confirmation of the tables, as well as for a melioration of them. We may be even sorry for astronomers devoting themselves to calculations so long and so fastidious, and yet obtaining only results so little different from those which we possessed before. But the tables of the sun constitute the foundation of all our calculations: they cannot be too frequently verified. It is particularly the duty of the members of the Board of Longitude to attend to this verification. It was on this account that M. Burckhardt has undertaken a still more laborious investigation of the tables of the moon, in order to obtain meliorations of the same kind. The very minuteness of these corrections is a most satisfactory proof of the singular perfection which astronomical observations and calculations have reached.

[To be continued.]

LXVIII. *Intelligence and Miscellaneous Articles.*

M. Millin, the learned Editor of the *Magazin Encyclopédique*, is at present engaged on a tour through Greece. He has recently transmitted to Paris an interesting account of the travels in Greece of two Danish Gentlemen, Messrs. Koes and Bronsted. They were at one period the fellow travellers of our countryman Mr. Cockerill. M. Bronsted undertook in 1812 to dig into the ruins of Cathaia in the island of Zea, near Attica. He obtained three female torsos, one of which is of most singular beauty: a torso of a colossal statue of Apollo Musagetes: the trunk of a horse, and several interesting inscriptions which were engraved on the pilasters of the temple. These inscriptions contain treaties of peace or alliance, written in the Doric language, with the Ætolians of Naupactos, the Athenians, and the Carystians of

 Eubœa.

Eubœa. These fine inscriptions, which furnish some novel ideas upon the sites of the four ancient cities of the island, are the property of M. Bronsted, who is well qualified to decypher them.

M. Bronsted in returning stopped at the island of Ithaca, so much celebrated by the father of Ionian poetry: on passing by Leucadia to Prevesa, he became acquainted with Ali Pacha, an old governor, full of energy and of a remarkable character. He traversed Albania, and was detained at Corfu by contrary winds. Here he found that abundance of medals had been obtained in consequence of the excavations ordered by General Douzelot.

RUINS OF POMPEII.

We translate from *L'Italico*, a valuable periodical work published in the Italian language in London, and conducted by the learned Dr. Augusto Bozzi Granville, the following account of an excavation made among the ruins of Pompeii on the 18th of March 1813. It is drawn up by an eye witness, and addressed in the form of a letter to Dr. Granville.

"They have commenced the execution of a great project here, viz. the clearing of the whole of the walls which surround Pompeii, and which are supposed to be about 1600 or 1700 toises in circumference. Great advantages will of course be derived in future excavations from the denudation of the walls. The streets which lead from the various gates will be more easily found, while there will be a greater facility in transporting the ashes and earth, and a guard may then be placed over the monuments to prevent dilapidations.

"The walls of this city are real fortifications: they are from 18 to 20 feet in height, and in some places higher: they are fortified at intervals with a kind of quadrangular towers partly destroyed; and they do not seem to have much exceeded the height of the wall. They are furnished with small gates, which seem to have answered the same purpose with those in modern fortresses. Certain it is that two of these already discovered were used by the brave inhabitants of Pompeii in their sorties against the troops of Sylla.

"The walls are twelve feet broad: they are ornamented, both on the side towards the city and towards the country, with parapets, which probably served in time of war as a security to the soldiery, and in peace as a promenade for the inhabitants. The parapets are furnished with loop-holes pretty close to each other, and with scuppers to carry off the water:—in several places there are flights of steps leading up from the city.

"The walls are not uniform, in consequence of the injuries they have sustained at various periods; they are mostly built of masses of fine stone four feet broad by five long, and two in thickness,

thickness, without lime, and yet well joined together, but so irregularly that the architecture is of the kind denominated *incertum*. If we are to believe that these are restorations made in the last days of the city, about the time of the siege of Sylla, and the earthquake A. D. 63, then the upper part of this description of architecture and the lower will be found to be more regular. Among a great number of these stones there was a monogram formed of an H and an E: on another a resemblance to the Greek L or cross formed of two Zs, similar to what we see upon paintings of ancient vases and in the monograms of medals. These probably were the characteristic marks of those who furnished the materials, while the Greek and Roman names which are so frequently met with, may have been those of the workmen, who probably did not think they would have been handed down to so late posterity.

Among the works on coins and medals which enrich the Cabinet of Medals at Milan, there is a work in Chinese, published in 1750 by order of Kien Long. It contains designs of upwards of 900 antique vases which strongly resemble the Etruscan, and they are of a very remote æra.

The Austrian Government has recently directed its particular attention to the diffusion of the means of education in Hungary. As the inhabitants of this kingdom speak four or five different languages, and their manners and religion are also different; three large schools have been founded, at which teachers will in future be educated, who will afterwards spread themselves over Hungary. These schools have been established at St. André, Pest, and Arad. Every branch of education necessary for the present state of European civilization will be taught at these seminaries, and the learning requisite for the Greek, Wallachian, and Illyrian churches.

A late number of a Journal entitled *Mines d'Orient*, published at Vienna by M. de Hammer, gives an extract from a curious letter respecting Arabian horses written by Dr. Seetzen, and dated Moka, 14th of November 1810. The writer maintains that these animals are much less numerous than has generally been supposed, and he mentions 5,500 as being the whole number of horses in all Arabia. He also combats the opinion generally entertained in Europe, respecting the beauty and good qualities of the Arabian breed of horses.

FINE ARTS.

Mr. West, the President of the Royal Academy, has we understand furnished Mr. Galt with materials for a history of his life. The

The work will embrace a vast number of original anecdotes of the most celebrated characters of Europe and America during the last sixty years. It will be particularly interesting to artists and students of art, not only for the development of the principles which the president has followed in his long and splendid career, but for his critical opinions on the remains of ancient sculpture and the great paintings in France and Italy. Mr. Galt, having himself visited many of the finest collections both in this country and abroad, will interweave in the narrative the observations of ingenious men, whom he has met with in the course of his travels. No work equally comprehensive, relative to the moral utility of the fine arts, and their actual state in the world at this time, has yet appeared. It will, besides, be as minute in the biographical details as if it had been executed by Mr. West himself.

Mr. Thelwall is preparing for the press, a Report of the results of his experience in the Treatment of Cases of Imperfection of the Roof of the Mouth, Uvula and Velum Palati, and other defects and malconformations of the Elocutionary Organs. Cases of this description, the author observes, have hitherto been regarded both by medical men and teachers of elocution, as totally incapable of relief: and though some ingenious dentists have attempted to supply the deficiences by artificial organs; these, it is asserted, have not only been found very imperfectly to answer the purposes of speech and intonation, but at the same time, in a considerable degree, to be both inconvenient and dangerous. Mr. T. maintains that in every one of the cases which have been submitted to his management,—the details of which will form the bulk of this little volume,—he had been completely successful, without any application of artificial apparatus, not only in producing a perfect intelligibility of utterance, but also in removing the offensive peculiarities of voice resulting from such malconformations. To this will be subjoined, Reports of several Cases of Amentia, or tardy and imperfect development of the faculties. A considerable portion of this volume was intended to have appeared in a pamphlet announced about this time last year, in reply to the assertion of the *Monthly Review*, that Mr. T's pretended discoveries were nothing more than *the most exploded of poetical errors*. Professional engagements having prevented at that time the prosecution of that work, the materials are now thrown into another form; and it may be interesting to inquire what connection there can possibly be between discoveries or principles by which such effects are to be produced, and any description of poetical errors exploded, or unexploded.

The work will be addressed to Mr. Cline, by whom several of the cases to be detailed have been referred to the Author's management.

Messrs. Kapoutanaki, of Smyrna, are preparing for publication a complete System of Universal Geography in modern Greek. That part which relates to the Ottoman Empire will be more copious than in any other publication known in Europe.

Dr. Wuttig, professor of chemistry and mineralogy in the university of Kasan in Russia, has announced the discovery of a new mineral, to which he has given the name of *miaszito*. According to his analysis, it is a compound of carbonate of lime and carbonate of strontian; it may therefore be only a variety of the mineral arragonite discovered by professor Stromeger.

Chevalier Cicognora, President of the Athenæum of Venice, has published the Prospectus of his History of Sculpture from its revival in Italy to the present time, in order to form a Supplement to the works of Winckelman and M. Leroux d'Agincourt. The work will be divided into six books. The first will relate to the origin of the art. The second will contain the history of those temples, ancient and modern, which are distinguished by ornaments of sculpture. And in the four subsequent the author will give the history of sculpture properly so called, divided into five periods: 1st, from Nicolo Pisano to Donatello; 2d, from Donatello to Michael Angelo; 3d, from Michael Angelo to Bernini; 4th, from Bernini to Canova; and 5th, to the present day.

NEW ZEALAND FLAX CULTIVATED WITH SUCCESS IN FRANCE.

In a letter from M. Faujas St. Fond to M. Thouin, inserted in the *Annales du Museum d'Histoire Naturelle*, he announces that the *Phormium tenax*, or New Zealand flax, which had hitherto never flowered in Europe, had given out flowers in the garden of M. Freycinet, the father of two officers who were in Capt. Baudin's expedition.

"The *Phormium tenax*," M. Faujas St. Fond adds, "was cultivated in the garden of M. Freycinet and in my own. We took care to cover it in winter; but as we were at first anxious to multiply our plants, we cut off shoots every year, which greatly impoverished the principal plants, and of course interrupted their flowering. At length, when we became anxious to see them flower, we reserved about a tenth, and left them to themselves.

"These plants soon increased prodigiously, and on the 10th of May (1813) M. Freycinet informed me that a very vigorous flower-stalk was shooting out from the centre of one of his strongest plants. Seven days afterwards this stalk was three feet high: on the 31st it was five feet six inches, and on the 7th of June six feet ten inches. On the 14th, its term of greatest increase,

increase, it was seven feet six lines; the stalk was then three inches and four lines in circumference at the base, and two inches and a half half way up. The flowers to the number of a hundred and nine are borne upon alternate peduncles, and have a pleasing effect. The colour of the flower is a greenish yellow, that of the stamina a golden yellow.

"I intend to make a drawing of the plant, in order that it may be compared with the figure published by Foster in Capt. Cook's Second Voyage. I have made some very strong ropes with the leaves, from which I obtained the flax by a very simple process. I shall send these ropes to the Museum of Natural History."

CITY OF LONDON TRUSS SOCIETY

For the Relief of the ruptured Poor throughout the Kingdom.

We give publicity to the following official statement respecting this useful and extensive charity with great pleasure. From the great number of persons among the labouring poor who were afflicted with hernia, and for whose relief no adequate provision existed, on the 14th of October 1807 Dr. Squire, Dr. Herdman, John Taunton, the Rev. H. G. Watkins, James Horwood, Michael Bartlett, Joseph Atkinson, John Middleton, John Gardner, and John Whitford, met at the City Dispensary, and formed themselves into a Society "for the Relief of the ruptured Poor throughout the Kingdom, the City of London Truss Society."

From this very humble beginning, the Society has increased in its means by the exertions of a few individuals, and is now extending relief to nearly 2,000 patients annually.

The following account of herniary cases, and their varieties, must be interesting to the philosophical as well as medical reader.

The great facility of obtaining trusses, and the liberality of the Committee in allowing Governors to recommend patients for relief, the expense of which often exceeds the amount of their subscriptions ten-fold, have contributed in no small degree to the extension of its benefits, whilst the strict œconomy observed in its expenditure has enabled them to meet the demands with but a trifling defalcation.

The little extra expense and the statement of accounts are perhaps unparalleled. The whole subscriptions from the commencement of the Society, which are brought into one view, will enable a liberal public to judge at once of the correctness with which its benevolence is applied.

The following statement of the situation and occurrence of hernia, at different periods of life, has been extracted from the register

register of the patients relieved by the City of London Truss Society, within the short period of six years.

In 6035 cases 4908 were males, and 1127 were females.

Males.	Females.			
1089	7	left inguinal	3079 inguinal	3586 single
1973	10	right inguinal		
29	216	left femoral	507 femoral	
38	224	right femoral		
1599	1	double inguinal	....	1737 double
30	107	double femoral		
19	259	umbilical	....	312
8	26	ventral		
17	24	have undergone operations, all except *one* have been completely successful		41
52	65	with umbilical hernia have been cured without trusses		117
53	7	with prolapsus ani		60
	181	with prolapsus uteri		181
1		with varix of the abdominal veins ..		1
4908	1127	——6035		6035

418	patients were relieved with trusses under 10 years of age.					
282	ditto,	between	10	and	20	ditto.
592	ditto,	———	20	and	30	ditto.
1029	ditto,	———	30	and	40	ditto.
1113	ditto,	———	40	and	50	ditto.
1124	ditto,	———	50	and	60	ditto.
768	ditto,	———	60	and	70	ditto.
278	ditto,	———	70	and	80	ditto.
30	ditto,	———	80	and	90	ditto.
2	ditto,	———	90	and	100	ditto.
5636						

Five females each had umbilical and double femoral hernia, 1 female had large ventral and double femoral hernia, 1 female had umbilical and ventral hernia, 1 female had ventral and prolapsus uteri, 5 females had umbilical and single femoral hernia, 1 female had right inguinal congenital hernia.

Six males each had left femoral and right inguinal hernia, 1 male had left inguinal and left femoral hernia, 2 males had left inguinal and right femoral hernia, 2 males had right inguinal and left femoral hernia, 2 males had double inguinal and right femoral hernia, 1 male had double inguinal and left femoral hernia,

hernia, 1 male had right inguinal and right femoral, 2 males each had double inguinal and double femoral hernia, 2 males had ventral and right inguinal hernia, 1 male had umbilical and left inguinal hernia, 1 male had umbilical and left femoral hernia, 5 males each had umbilical and double inguinal hernia; 402 patients had congenital hernia; 1 male had very large varicose veins on the abdomen.

From the most accurate estimation, it appears that this malady exists in one person in eight through the whole male population of this kingdom, and even in a much greater proportion among the labouring classes of the community, in manufacturing districts, particularly in those persons who are employed in weaving, or on the water as boatmen.

In one district in the western part of England, comprising 200,000 inhabitants, it has been ascertained, by actual observation, that upwards of one in five of the whole population labour under this malady.

JOHN TAUNTON,
Surgeon to the City of London Truss Society, the City and Finsbury Dispensaries, and Lecturer on Anatomy and Surgery.

21, Greville-Street, Hatton-Garden, 30th Dec. 1813.

Meteorological Observations made at Cambridge from March 15 *to* 31, 1814.

March 15 to 26.—The frosty and winterlike weather with snow showers and easterly winds continued till the 20th, when signs of a change appeared. From this time the air got continually warmer, features of *cirrocumulus* again ornamented the sky, and foreboded the return of spring. The great change of temperature about the 23d, the fine warm air and south-west wind of that day with beautiful forms of the *cirrocumulus*, the opening of crocus and other spring flowers, and the singing of the birds, and busy appearance of rooks constructing their nests; were circumstances so rapidly supervening on a long protracted and unseasonable frost, as to have a very pleasing effect, and to put one in mind of the sudden return of spring in the northern countries of the continent. We had a fine warm shower in the evening of the 26th.

March 27.—Very fine clear day and westerly wind, with beautiful features of *cirrocumulus* and *cumulus*.

March 28.—Fine warm day, *cirrus*, *cumulus*, &c.

March 29.—A veil of cloud obscured the sky, with westerly wind.

March 30.—Calm day: early in the morning a white *stratus* obscured the lower atmosphere; when it cleared off, and the day became fine, *cirrus* was seen aloft and light *cumuli*: towards two

two o'clock the *cirrus* disappeared, and the *cumuli* became larger, and approached in their nature to *cumulostratus*. Fine wholesome warm day. Therm. 58° in the shade. Wind westerly*.

March 31.—Cloudy all day, with some small rain and wind.

Observations made at Clapton from April 1 *to* 11.

April 1.—Cloudy morning, fair at times in the day. Wind southerly.

April 2.—Fair. *Cirrus, cumuli*, and other clouds; warm and wholesome air and clear night. Wind southerly.

April 3.—Fair; cloudy at times; fine afternoon, breeze from SW. *Cumuli* and some *cirrus*.

April 4. Cloudy morning and N wind, but still warm. The wind became easterly, and much *cirrocumulus* appeared in the masses of cloud in the evening, which was warm. Therm. at midnight 42°. Barom. 29·68.

April 5.—Fine day; wind northerly in the morning; *cumuli* and *cirri*; by night tufts of *cirrus*.

April 6.—Wind SW variable and gentle; a thick wet mist or fog in the morning (modification probably of) *cirrostratus*: when it cleared off, a fine day with *cumuli*, though the atmosphere was still rather hazy. Therm. at 11 P.M. 43°. Barom. 30·10.

April 7.—Fair day; fog early, and then *cumuli*. Therm. in the day 58°, 11 at night 44°.

April 8.—Mist early; then a few *cumuli* succeeded by SE wind, and very clear dry air: indeed the sky was quite cloudless. Therm. in the day 58°, at 11 P.M. 41°.

April 9.—Wind SE; clear sky with features of *cirrus* and other very various appearances; dry air and very clear distances.

April 10.—Clear day; exact *cirri* of various figures, and rapid changes as usual in this kind of weather. Wind calm and easterly. Red sunset.

April 11.—Clear day, but of a different kind from yesterday; no clouds except, a few small *cumuli*; clear sunset, but not much coloured.

* The daws, (*Corvi monedulæ*) soared round and round at a very great height in the air, and the raven croaked aloft: all signs of fine weather.

Corpus Christi College, Cambridge,
April 12, 1814.

THOMAS FORSTER.

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For April 1814.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
March 27	46	55	44	29·89	10	Fair
28	44	55	43	·78	14	Fair
29	45	54	39	·68	0	Rain
30	38	57	45	·85	32	Fair
31	45	56	47	·80	0	Rain
April 1	49	56	47	·69	27	Fair
2	50	56	45	·40	25	Stormy
3	47	55	43	·43	42	Fair
4	43	56	45	·62	40	Fair
5	42	54	46	·72	46	Fair
6	47	58	47	30·00	42	Fair
7	47	60	46	·10	43	Fair
8	42	59	43	·25	40	Fair
9	43	58	46	·20	41	Fair
10	46	57	42	·15	51	Fair
11	45	60	48	·04	50	Fair
12	46	66	57	29·90	57	Fair
13	57	72	58	·72	71	Fair
14	56	69	58	·63	75	Fair
15	59	66	55	·50	42	Cloudy
16	57	67	56	·48	36	Showery
17	56	62	56	·59	34	Showery
18	54	63	52	·64	37	Showery
19	56	60	54	·76	48	Fair
20	54	62	55	·74	42	Cloudy
21	54	58	46	·78	40	Cloudy
22	46	56	47	·89	42	Cloudy
23	47	55	45	·98	41	Cloudy
24	44	50	42	·82	36	Stormy
25	44	51	43	·86	24	Stormy
26	43	52	43	30·09	31	Cloudy

N.B. The Barometer's height is taken at one o'clock.

LXIX. *Improvement in the Axis of large Water-Wheels, to prevent the Gudgeon from getting loose in the Shaft, or to repair it when damaged.* By Mr. ROBERT HUGHES, *of Ruabon, Denbighshire**.

SIR,—I BEG leave to observe to you, that from the common mode in which the gudgeons, for cranes or water-wheels, are now fixed, they are liable to heat in working, and soon become loose; and by attempting to secure them with wedges, the shaft gets split, injured, and rendered useless.

To obviate such inconveniences, and even to repair an injured shaft, I have invented and practised a plan, which has perfectly succeeded, and of which I have sent a model to the Society.

Upon this plan, more firmness and security is given, without weakening the shaft by the deep mortices usually cut therein for fixing the gudgeons. My invention is also cheaper than the old method, in which the hoops alone cost about four guineas, and the cast-iron gudgeon 2*l.* 12*s.*; whereas on my plan the expense of the whole will only be 4*l.* 16*s.* If necessary, I will furnish certificates from persons who have made trial of my invention.

I am, sir,
Your humble servant,
ROBERT HUGHES.

Ruabon, Feb. 12, 1812.

To C. Taylor, M.D. Sec.

Certificates.

On the 14th of June, 1811, Robert Hughes, of Ruabon, instructed me in making a cast-hoop gudgeon, for a tumbling shaft of a large and powerful water-wheel for grinding clay for bricks, tiles, &c. which I have found to be a great improvement, as witness my hand this 3d day of October 1812.

THOMAS EVANS.

Trefynant, Ruabon, Denbighshire.

This is to certify, that Robert Hughes, of Ruabon, formed a new construction of gudgeons, for the shaft of a water-wheel he built for me, in the year 1806, which wheel is 28 feet diameter. These gudgeons took in the whole body of the shaft, and wedged the outside. I have not been put to any expense since the making thereof, nor am likely so to be until the shaft decays by time. In short, I cannot speak too much in his praise, with regard to his ingenuity and workmanship. Witness my hand, at the Pant Mill, August 20, 1812.

THOMAS MANLEY.

* From *Transactions of the Society for the Encouragement of Arts, &c.* for 1813.—The Society's silver medal was voted to Mr. Hughes for this communication.

The

The wood shaft of a water-wheel, 18 feet diameter, was so worn and decayed at one end, by frequent putting in and wedging of the gudgeon, as to render a repetition of that process impracticable. Robert Hughes, in this difficulty, contrived a cast-iron gudgeon, with a hoop to it, which inclosed the end of the shaft; it was put on and fastened, and has worked steadily ever since. I am informed, that he puts the hoop gudgeon now on new shafts, and it seems to me a great improvement upon the gudgeons formerly in use.

Ruthin, Sept. 14, 1812. JOHN JONES.

Reference to the Engraving of Mr. ROBERT HUGHES's *Method of fixing Pivots, or Gudgeons, into the Shaft or wooden Axis of a Water Wheel, which will prevent the Danger of their getting loose in the Shaft, and permit their being repaired when damaged.* Plate V. Fig. 1, 2, 3.

The ordinary method of fixing gudgeons into wooden shafts, is to have an iron cross, formed on the end of the gudgeon, which is let into the wood, to a considerable depth, and is held fast by small wedges, driven in round it, hoops being fitted round the outside of the shaft, to prevent the wedges splitting it. The defects of this method are, that the shaft is much weakened, by the cutting into it; and the constant strain of a heavy wheel, always acting in different directions, causes the wedges to become loose, and at length fall out; the failure of the gudgeon is frequently attended with more serious damage, for the wheel falling upon the bottom of the race, generally breaks the rim and buckets by its own weight.

The improvement made by Mr. Hughes, consists in applying a cast-iron box to the end of the shaft, and to this the iron cross of the gudgeon is screwed, so as to fasten it more firmly than by the old method, and without weakening the wood by cutting it away. AA, fig. 1, of Plate V. represents a portion of the end of the wooden shaft, which is of an octagonal form, and has the water-wheel fixed upon it; it is long enough to reach across the pit, in which the wheel works, and having a gudgeon at each end, is supported and revolves upon them in proper bearings. BB is the cast-iron box, fitted fast upon the end of the shaft, and being wedged tight, preserves the wood from splitting as effectually as any hoops can do; upon the end of the box is a projecting flanch, *aa*, and in the face of this four grooves or notches are made, for the reception of the arms of the iron cross *bb*, *dd*, which is part of the gudgeon C, on which the shaft revolves; this cross is firmly attached to the box, by four screw bolts, which pass through the flanch, and also through the ends of the arms of the cross, having nuts screwed on the outside to make

make all fast. The section, fig. 3, shows the cross *bb*, and box BB, separated, to explain the manner of fitting them together, the inside face of the cross having projections *ee*, which enter the end of the box, and keep the pivot in its true centre: thus the bolts have only to hold the gudgeons fast on the end of the box, the principal strain being taken off by this manner of fitting the cross into the box.

When the gudgeon of a wheel, fitted on this plan, becomes worn out, so as to require a new one, it can be removed, by taking off the four nuts, and a new one applied.

The cross and gudgeon, being of small dimensions, admit of its cylindrical part C being very conveniently turned in the lathe.

LXX. *Description of a Carriage-Wheel Guard, by which the Wheel is retained safe upon the Axis in case of the Linch-pin coming out. By Mr.* John Padbury, *of Speenhamland**.

Sir,—From a conviction of the good wishes which the Society, for which you act as Secretary, ever feel for ingenuity, in its various branches, I am induced to lay before you a plan of my invention, for the more effectually giving safety to the wheels of carriages, to prevent their breaking, or to avoid any mischief from loss of that very considerable support, the *Linch-pin*, on the dependence of which the lives and limbs of so many are continually exposed.

The plan I now offer for the investigation of the Society, is calculated to obviate the danger which may arise from such an accident; the great repute it has obtained, and the almost general application of it by the coach proprietors of the Bath and other roads, together with the prevention of many accidents since its adoption, will, I flatter myself, plead a sufficient apology for my offering it to your notice.

I am, sir,

Your obedient humble servant,

Speenhamland, Oct. 1812. John Padbury.

To C. Taylor, M.D. Sec.

The following gentlemen, residing in this neighbourhood, have inspected the apparatus, and much approve of it. They have

* From *Transactions of the Society for the Encouragement of Arts, &c.* for 1813.—The silver medal of the Society was voted to Mr Padbury for this communication.

 done

done me the honour to affix their names in testimony of its utility.

Joseph Andrews,	Charles Dundas,
William Hallett,	John Disney, Jun.
James Croft,	Anthony Bacon,
John Wells,	Francis S. Stead.
Richard Boucher,	

Reference to the Engraving of Mr. John Padbury's *Guard for a Carriage-Wheel, by which the Wheel is retained safe upon the Axle, in case of the Linch-pin coming out.* Plate V. fig. 4, 5.

This contrivance can be applied to any carriage, as it does not require any part of the carriage, or axle-tree, to be altered, further than the addition of an extra iron hoop upon the inner end of the nave of the wheel.

This is fully explained by figs. 4 and 5, Plate V, the former being a section, lengthways, of the axletree, and the latter a transverse section; AA represents the iron axletree of a carriage, bolted beneath the wooden bed BB, or frame, of the carriage; CC is the wheel; DD its nave, the axletree box *dd* being fitted through the centre of it as usual; *a* is the linch-pin, passing through a mortice in the end of the axletree; EE is a circular box, fastened to the bed BB of the carriage, and surrounding the inner end of the nave of the wheel, to prevent dirt from getting into the axletree; the extra hoop, before mentioned, which is fixed on the end of the nave, is contained within this box, and runs round without touching it; it consists of a strong hoop *bb*, fixed fast upon the nave, and having a flat circular ring, formed out of the same piece with it: the latter, when fixed, makes a circular projecting rim, upon the end of the nave, as the figure plainly shows.

The same clasp band F, which, by means of two nuts beneath, fastens the axletree AA to the bed of the carriage, also fastens down a piece of iron G, upon the bed B; in the extremity of this piece of iron is a hole for the reception of a screw *e*, the point of which enters within the box EE, and comes before the projecting ring *bb*, so as to effectually prevent the wheel from coming-off its axle, even if the linch-pin should by accident drop out: the point of the pin *e* is so situated as not to come in contact with the plate *bb*, when the linch-pin is in its place; therefore it occasions no friction to retard the motion of the carriage.

When it is required to take off the wheel, for the purpose of repairs, or for greasing the axletree, the screw *e* is taken out, and the linch-pin being removed, the wheel can be taken off.

In the xxviith volume of the Society's Transactions, at p. 147, will be found a description of an invention, by Mr. J. Varty, of Liverpool,

Liverpool, to be applied to the linch-pin of a carriage, to prevent the wheel coming off its axle, if the linch-pin should drop out; this contrivance being wholly applied at the extremity of the axletree, close to the linch-pin, it may be used in combination with Mr. Padbury's invention, and would then render a carriage so secure that it would be scarcely possible for the wheel to come off, by any accident short of the breaking of the axletree: and even then there would be a chance of the wheel being retained, in the circular box EE, by the point of the screw *e*, so as to sustain the carriage from falling, until the horses could be stopped.

LXXI. *Notes and Observations on Part of the Eleventh and the Twelfth Chapters, and Appendix, of Mr.* Robert Bakewell's *"Introduction to Geology;"—embracing incidentally, several new Points of Geological Investigation and Theory. By Mr.* John Farey, *Sen., Mineral Surveyor.*

[Concluded from p. 261.]

Notes, &c.

[P. 274] l. 8, of Craven, in Yorkshire ‡.—‡ The north-western parts of which, at least, seem to have the same Limestone Rock as in North Wales, and in Shropshire, as Mr. B. is said to have represented, P. M. xxxix. p. 236, but not the same with the lowest of those in the Peak of Derbyshire, I think, see my 1st Letter, vol. xlii. p. 59, and 3d Letter, p. 170.

l. 12, exclude the coal **.—** The mode in which this exclusion happens, is very conveniently passed over in silence, see a Note in my 2d Letter, vol. xlii. p. 105.

l. 18, a few miles only††.—†† Query,—see my 1st Letter, vol. xlii. p. 57, and notes on p. 257.

P. 275, l. 14, Some traces of the rocks*.—*The great improbability of this supposed identity of the Peak Limestones, with those of Ticknall and Grace-Dieu, is heightened, by another supposition in p. 166, of Wild-Park, Breedon and Clouds-hill, contorted magnesian Limestones (intermixed with these) being identical with the yellow Lime Rock, on the eastern side of Derbyshire, as observed in my Note on p. 167, see also my 2d Letter, vol. xlii. p. 106.—" It will scarcely satisfy the curious to be told, that these lime rocks are mere anomalous masses," Mr. B. p. 46, in your xlth volume.

l. 26, *west* side of Anglesea †.—† The Coal-field in Anglesea is situated on its *south-east* side, rather, (see vol. xlii. p. 57): here three seams of Coal of good caking quality have been wrought, at different times, in the parish of Llanfihangel Esceifiog, for ages past: the uppermost of these is

[P.275] a soft Coal, generally of five yards thick, the next a hard Coal, mostly of two yards thick, and the lower one is usually three quarters of a yard thick. Clunch or fire Clay occurs under each of these three Coals, as usual (Rep. i. 179 and 446 and P. M. xliii. p. 28), and Shale, Bind and Gritstone beds on and between them; altogether these *Coal-measures* seem about 150 yards thick: below the three-quarter seam, there is a thickness—perhaps 40 yards, of *barren Coal-measures*, in which, although the appearances are very promising and have induced several trials, seams of sufficient thickness to work, are rarely met with. Below this occur what I have denominated the *Floor Rocks* of the Anglesea Coal-trough, (vol. xlii. p. 356 and 360) and seem together to be about 90 yards thick, consisting of a variably and very coarse Gritstone with Jasper fragments in it occasionally; below which is a gray Limestone Rock (very cherty and bad in quality in Llanfihangel); then the middle coarse Gritstone Rock, often containing huge massesof Chert (as Graig-fawr, vol. xliii. p. 126) and Jasper fragments, coaly masses, &c. Below this the black Limestone or (Kilkenny) Marble Rock in thin beds, occurs; and then the lower variously coarse Grit Rock, graduating locally into Conglomerate, see my Note on p. 202; and then the coarse *Slate* succeeds. Both the Limestone Rocks above mentioned, are I believe included in the Halkin or highest of the *three* Limestone Rocks mentioned, vol. xlii. p. 53, 59, &c.; in which case, they would with more propriety perhaps be called *four* Rocks of Limestone, that surround, and basset on more than three sides of the slaty mountains of Wales. And, if these are classed with the four Limestones of the Peak of Derbyshire, what very anomalous substances must we not admit, in the places of its three Toadstones?.

277, l. 8, Hills in Derbyshire are *. — * Blakelow-stone is probably the highest; higher than Holme-moss, see my Notes on p. 272 and 278: Whin Hill and Mam Tor here mentioned, are inferior in height to several that surround them; Lords Seat near, at hand, over-looks them both.

278, l. 21, has omitted the height *.—* Not exactly so, since the *height* of Axe-edge N hill is said to be 1875 feet, Rep. i. 17; at the time my volume was published, the List of *altitudes* in the "Account of the Trigonometrical Survey from 1800 to 1809," had not appeared: and even in this work, some difficulties arise, from *Axe-edge* and *Lords-Seat*, distant 49,212½ feet, (p. 128) and bearing about 31° 42′ 13″ E of the N, being *each stated to be* 1751 *feet* above the sea (p. 302 and 308); although, at Axe-edge, Lords-

Seat

[P.278]Seat appeared depressed 3′ 53″ (p. 66), and at Lords-Seat, Axe-edge appeared depressed 5′ 26″; and some calculations will be necessary (which I have not now time for), to decide, which height is erroneous: that one of them, or of the depressions, must be so, is too apparent.

The other altitudes mentioned in this work, of Hills contained in my List, Rep. i. 16, and P. M. xxxvii. p. 161, are *Alport* (or Orpit) Hill of 1st Grit 980 feet, *Bardon* Hill of coarse Slate 853 feet, *Bradfield* Moor (or Point) of 1st Grit 1246 feet, *Hathersage* Ridge of 2d Grit 1377 feet, *Holland* (Hollin or Holan) Hill of Red Marl 487 feet, *Holme* Moss of 2d Grit 1859, *Mole-copt* of coarse Grit 1091 feet, *Sutton* (Sherwood) Forest-Hill of Gravel 600 feet, and *Weaver* Hill of 4th Lime 1154 feet.

l. 24, over Kinder Scout †.—† This great and irregular Hummock of 1st Grit (Rep. i. 226) has no particular *summit*, that is very decidedly higher than several others on its edges, much less, " 400 feet" higher, than any line which could be traced " over" this Mountain: Mr. B's Work would have suffered nothing, from the omission of the whole of this paragraph: and a piece of injustice to an Engineer of eminence, in publishing this statement would have been avoided: since I cannot doubt but Mr. Brown has been completely misunderstood by Mr. B. because, on reference to a " Plan of the *Ashton under Line*, *Peak Forest*, *Huddersfield* and *Rochdale* Canals," prepared by Mr. Brown, for publication, by Mr. J. Cary, some years ago, whereon the *levels* are stated, of the existing Canals, it appears, that from the Duke of *Bridgewater's* Canal at Manchester to the Bugsworth branch of the *Peak Forest* Canal, the *rises* are 75f. 7in. 114f. 6in. 48f. 0in. and 212f. 0in; to which adding 82 feet for the Locks at Runcorn into the Tide-way in the *Mersey*, we have 532f. 1in. for the elevation of Bugsworth Wharf above the Sea. It further appears from the Map referred to, and the abstract of a Survey that was *made under Mr. Brown's direction* in 1810, contained in Mr. Rennie's printed Letter of the 26th of October 1810, on the intended " *High-Peak Junction* Canal," that the further rises of 129f. 0in. and 189f. 9in. attain *the summit level* of this proposed Canal, which passing from near Malcalf through a proposed Tunnel *under Bowden-Edge*, (which is between Lords-Seat and Kinder Scout) emerges again at Dale-end in Ecal Valley on the S side of Kinder Scout mountain, and continues thence to skirt its southern flank to Nether Booth, before the lockage is to begin, for descending towards the eastern Rivers, (see Rep. vol. ii. chap. xvi. sect. 3). This

[P.278] This summit level, at 809f. 10in. above the Sea, is probably much more than "400 feet" lower than the bold edges of the Kinder Scout Hummock, that over-look it, since this would make their elevation only 1210 feet, instead of Mr. B's 2100 feet! or instead of 1770 or 1800 feet as ought I think to result, according to my comparative estimation, and recollection of the heights of these hills, and supposing that we can rely on the height of Lords Seat, as stated by Col. Mudge: and because Mr. Nuttall in 1789 found the summit of the Buxton and Manchester Road to be 1198 feet above the *Cromford* Canal, which probably is about 248 feet above the Sea, and gives this point of the Road an elevation of 1446 feet.

279, l. 15, *it* rises to the surface*.—* Quere 1st or 4th Limestone?.—See my 1st Letter, vol. xlii. p. 59, 2d Letter, p. 112, and 3d Letter, p. 170, and Note on p. 274, l. 8.

These Limestone Rocks Mr. B. represents as *transition* Rocks, (see Rep. i. 298 Note); yet a very principal of the Anglo-Wernerian Writers, after admitting that *some petrifactions* do occur in transition Limestone, lately said (Ann. of Phil. iii. 116 Note), "but hitherto as far as I know, *no shells* have been found in that Rock, nor anything else, except *madrepores* and *othoceratites;*"—let any one who has read William Martin's "Petrificata Derbiensia," say, whether this Wernerian dogma, will at all apply to the Peak hundreds of Derbyshire? The *Geognostic* assertion, that particular *Classes, Orders,* or even *Genera* of organic remains, characterise the Formations, as to their relative *ages,* or priority of deposition, and the consequent order of the superposition of the strata, (see Ann. of Phil. i. 205): appears to me vastly inferior, either in truth or practical utility, to the discovery by Mr. *William Smith,* about the year 1792, (Rep. i. 109), made without communication with any *Wernerian pupil* (if any such had appeared in this country at that time?) as to particular *species* of Shells, &c. that indiscriminately belong to *different genera, orders,* &c. distinguishing, in a very marked and satisfactory manner, the greater part of the strata of England;—has Mr. Jameson told us in his account of "the Geognosy," the particular *species of shells* and other organic remains, or attempted it, that characterise the numerous strata of Scotland?, whence nearly all his British examples are drawn, or was he able to give such tests of his Geognostic deductions, as to any other country?.

281, l. 1, red clay*.—* This Ironstone (like that near Coleford in Dean Forest and Merlin Griffith in Glamorganshire,) occurs

[P.281] curs in Veins or Cavities in (probably the same, or Halkin?) *Limestone Rock*, P. M. xxxviii. p. 274, and Rep. i. 402 Note, and p. xlvii.

l. 9, the subjacent Rock †.—† Rep. i. 280.

l. 12 and 13, rests upon slate ‡.—‡ This is an assumption supported by no just analogy, see my 1st Letrer, vol. xlii. p. 59, and Notes on pp. 274 and 279.

l. 15, 250 yards**.—** 350 yards, and probably much more; the seven Rocks, four of Lime and three of Toadstone.

l. 17, varying much ††.—†† Rep. i. 238 and 276.

l. 20, much thicker ‡‡.—‡‡ Rep. i. 280.

282, l. 8, our present manager*.—* Mr. Theodore Silverwood, whose "Section" I have noticed, Rep. i. xxiv. but have therein, hardly done sufficient justice to his great kindness to me when on my Survey, or said all which I ought, of such a benefactor to the Science of Geology, in my 2d Letter, vol. xlii. p. 109; and in the following page, I have omitted to mention, that the Section-line I have suggested, would, on the west side of Golden Valley, pass along the line of the *Butterley Tunnel* on the *Cromford* Canal, whereby an excellent Section that already exists, of 2978 yards in length of this line, in the possession of William Jessop, Esq. of Butterley Hall, would be brought into use, and rendered extremely interesting. When I was at Butterley, Mr. Jessop kindly lent me this Section to take with me to Mr. Mushet's, where it remained some time, and, if I mistake not, Mr. Silverwood made and retained a copy of it, which unfortunately I had not leisure to do myself, but was content with noticing an abstract of its results, the greater part of which will appear in my account of this Canal, in the 3d volume of my Report now printing.

283, l. 13 and 14, to reach the coal strata*.—* *Seven hundred yards* is very far short of the probable distance, of even the Red Marl, (Rep. i. 116) below London, see my Note on page 259.

l. 21, in Mr. Townsend's estimate †.—† Which, of "the Derbyshire strata," has Mr. Townsend included in his account, quoted by Mr. B. p. 260?—Mr. T. says a great deal in his work, on very slender grounds, about the "Mountain Limestone" of Mendip and its neighbourhood, being the same as in the Peak of Derbyshire, yet he expressly says, it is a different stratum from the "Lyas," which last he states only at 20 yards thick, and the Mendip Rock is not therefore included in page 260. I think, that these Mendip strata are greatly thickened beds of the Lias; or perhaps of some of its

[P.283] its Marl beds, in a more perfectly calcareous and stony state, than usual, see my Note on p. 12.

284, l. 4 and 5, one mile and a quarter*.—* Three Miles, P. M. xxxv. p. 130.

l. 8, I have before said†.—† See p. 262, and my Note thereon.

l. 10, beyond the Peak‡.—‡ Not on the surface; because thrown down (comparatively speaking) by the great Limestone Fault, see my 2d Letter, vol. xlii. p. 112, and Note on p. 275.

l. 12, appears to terminate**.—** See my Note on p. 275.

l. 17, considered as *primary*††.—†† Although I never consider any Rocks in this light, for the reasons stated in P.M. vol. xxxix. p. 29, my Note on pp. 43 and 44, and elsewhere, yet I think I perceive (from pp. 57 and 85), that Mr. B. here *means* "*Transition.*"

l. 18, extend about ten miles ‡‡.—‡‡ See Map in Rep. p. 97 and p. 151.

l. 25, admit its existence***.—*** See Rep. i. 166, and my 2d Letter, vol. xlii. p. 108.

285, l. 6, Basaltic rock near Nuneaton*.—* See also p. 290. The Basalt at Griff-hollow, Marston, Harthill, &c. occurs on the Bedworth Coal strata, I believe¶, and so does that of the Clee-Hills on its Coal-field (and the Rowley Hills also on another Coal-field, which circumstance Mr. B. has overlooked); and in assimilating these Basalts with the coarse Slate of Charnwood Forest (as I have done, Rep. i. 155), both here and in page 290, Mr. B. was not aware probably, that he was indeed, granting my very heterodox position, as to the Ashby-de-la-Zouch Coal-measures *passing-under*

¶ Several months after this was written, my friend Mr. Benjamin Bevan (in April 1814) informed me, that he had obtained Sections of the strata in the Bedworth Coal-field, and had certainly ascertained, that the *regularly stratified* Red Marl and imbedded Basalt or Greenstone, &c. are *laid unconformably over the dipping Coal-measures*, in this case; in Somersetshire the same was long ago asserted by Mr. John Strachey, in No. 360 and No. 391 of the Philosophical Transactions; but after many conversations with Mr. Smith and others, on the facts of the Somersetshire Coal-field, and very anxious inquiries, as to the correctness of Mr. Strachey's statements, I never could obtain satisfaction, as to the Red Marl, &c. being *regular and undisturbed*, which had been proved by sinking through it, to overlie the Coal-measures *in an unconformable manner*: and therefore I have always doubted the correctness of a fact, so very contrary to my own observations, and that of hundreds of others, in so many other situations, and so I have expressed myself in my Notes on pages 45 and 98:—my zeal and industry shall however be increased, for inquiring fully into this important fact, and as to the extent of its operation in the structure of England, and I shall be thankful for the assistance of your correspondents herein.

Charnwood

[P. 285] Charnwood Forest—"truth will now and then out;" see my Note on p. 67.

I long to examine the Malvern Hills, to see what I can make of them, and of the Coal-measures in their vicinity, of which my Friend Mr. Mushet speaks, P. M. xl. p. 54, and the Wrekin and Cardock Hills, &c. p. 293, and the central parts of the South-Wales Coal-field, P. M. xl. p. 52.

l. 9, and Wales †.—† I am sorry that I cannot safely go thus far at present with Mr. B., unless he can show, that Basalt or its asociates, Porphyry, Sienite, &c. see p. 97, and 289, (perhaps the "Pennard" Rock of Mr. Mushet, P. M. xl. p. 52?) are found on the central parts of Mr. Edw. Martin's great Coal-basin, (p. 268 and 299): not being able to admit, that the porphyritic green-stone on Cader Idris, p. 112, 189 and 297, *overlies any Coal strata*, that I know of, in their vicinity. To me it seems not improbable, that the Coal strata of South and of North Wales, of Shropshire and Dean Forest and those of Anglesea, (see my Note on p. 275) and even those of Antrim ¶ and Leitrim, Roscommon, Sligo ¶¶ and Kilkenny, were once spread, even *over* Snowden, &c. &c. and were absolutely connected, before stupendous denudations separated them! see P. M. xxxvii. pp. 441 and 442.

l. 10 and 11, nearly horizontal ‡.—‡ Rep. i. 152 and 147.

l. 11, sandstone **.—** Red Marl with its gritstone and other imbedded substances, Rep. i. 154.

l. 18, evidently formed from them ††.—†† I maintain the Red Marl to be of the very same age with its various imbedded substances (even Granite, as Mr. Mushet mentions, P. M. xl. p. 53, and see Rep. i. 280), and so of any other stratum with respect to its imbedded masses, whether Chalk and Flints, or micaceous Sandstone and Conglomerate, &c. see my Note on pp. 43 and 44.

l. 20, of fragments of these Rocks ‡.—‡ From this passage, any one would suppose, that fragments of Sienite and Slate were found in vast quantities, forming Breccia, or Conglomerate and Gravel, to the NW of Charnwood Forest; but no such thing occurs, I think I can assure them, only occasionally, strata of variously coarse Gritstone, whose exposed surfaces are liable to be mistaken for Gravel Rocks, (Rep. i. p. xiii.); and on further search in Mr. B's Book,

¶ The information accessible to me in 1812, seemed to leave it doubtful, whether the *great* Basalt of Antrim overlie its Coals, or not?, P. M. xxxix. p. 353, and such doubt still remains; but none I think, that there is a Basalt stratum on them.

¶¶ Sir H. Davy in his Agricultural "Chemistry" informs us, that many of the highest hills in these Coal-fields "have basaltic summits."

these

[P.285] these multitudes "of fragments" are *supposed* to be derived from *supposed* veins of white Quartz, (p. 287 and 188) and by *suppositions*, equally probable, these fragments of quartz, in travelling a Mile, or 20 at most, within the distance here alluded to, are *supposed* to be in a great degree if not perfectly *rounded!!*, see my Note on page 188.

286, l. 4, of sandstone *.—* Red Marl, see my Note on p. 67.

l. 7, rests on the flanks †.—† When I examined this Forest, it was in too much haste, as observed in my 2d Letter, vol. xlii. p. 116; had it been situated *in Derbyshire*, it would have had a far more minute examination, and I should have been able to speak rather positively, as to what thick Rocks would be seen in ascending any particular Brook (except perhaps in some Coal-districts,) here I cannot be equally confident. In page 158, vol. i. of my Report, I have mentioned the appearances, as justifying the inference, that the Grace-Dieu limestone rock *passes under the Forest Slate:* and when I now recollect, the confidence with which Mr. B., *from Theory only*, has asserted, in his Note on page 266 (see my Note thereon), that similar discoveries of *lower strata* might be made, by ascending the ravines in the North Eastern Moorlands of Yorkshire, he must excuse me for continuing to think, that he is not less mistaken as to the Grace-Dieu ravine, until I shall happen to have the opportunity of examining the spot more minutely, or of learning the observations of others, who have so examined it.

l. 15, its termination at this place ‡.—‡ See my Note on p. 275.

l. 16, singularly contorted **.—** Rep. i. 159.

l. 20, nodules of lead ore ††.—†† Such bolders are found occasionally, where no adjacent Limestone Rock and Lead Veins, can easily be supposed to have disappeared, as in the Gravel Pit at Wyaston, Rep. i. 372, &c.

287, l. 5, at Barrow on Soar *.—* Blue *Lias*¶, Rep. i. 115. How Mr. B. makes this place to be "on the other side of

¶ This remarkable Limestone Rock (see top of Mr. B's page 16), from its pozolanic property, the fossil *Bones* and flat scaly *Fish*, the *Pentacrinus*, &c. &c. that it contains, seems to me, from the translation of M. Cuvier's Geology, p. 260, to enter the Coast of France at the mouth of the Seine, and to pass inland by a circuitous route westward, to Alençon in the department of Orne, and perhaps in the dep. of the Mayne and Loire, &c. If I am right in these conjectures, the fine Bath *Oalite* near Caen in the department of Calvados, whence it is said that Henry the Seventh's Chapel in Westminster was built, probably occupies the tops of the hills (unless a depression or trough occurs there?) as it does to the E and SE of Bath, (see my 2d Note on page 167); the Bridge in which last City, stands on the eastern edge of the ring of Lias, that surrounds another of Red Marl, including and overlieing a small denudated Coal-field, occurring between Bath and Keynsham.

the

[P.287] the forest" from the Canal or Water-level belonging to the *Leicester* Navigation (in the cutting of which, nodules of Lead Ore were found, as mentioned in his Note on the preceding page) I am at a loss to conjecture; when they are more properly *on the same side*, and the general direction of this crooked Canal points for Barrow on Soar. Without doubt, I think, the Lias strata, on Red Marl, once overlaid the whole of Charnwood Forest (but not any of the Limestones of Derbyshire) before the Denudation thereof, (P. M. xxxvii. p. 442); and it is perhaps equally probable, that the bolders of Lead Ore mentioned, may be moved from the edges of the Lias strata, or some others further eastward, and lodged in the Gravel on the Forest, as that they are the produce of thin and perhaps unconnected veins of Lead Ore, in the coarse Slate, of the spots where they are found. Spars of any kind, are rarely found, in many of the Lead Mines in Cardiganshire, owing to which, the discovery and tracing of Veins there, is far more difficult than in the Peaks of Derbyshire.

l. 18, the slate quarries†.—† Rep. i. 153.

288, l. 3, erroneously stated*.—* Rep. i. 19.

l. 5, the *beds* are more regular†.—† In sudden Ridges, with various apparent dips, at Swithland, &c. Rep. i. 154.

l. 15, nearly at *right angles**.—* Said to be 60° at page 87, and in "an opposite direction," P. M. xl. p. 47, see my 2d Letter, vol. xlii. p. 116.

289, l. 11 and 12, these rocks are cotemporaneous*.—* Rep. i. 153, Phil. Trans. 1811, and P. M. xxxix. p. 28, see also my Notes on pages 43 and 285.

290, l. 18, inclined as if it rose*.—* This I was unable to discover, Rep. i. 153, and p. 155 Note.

l. 20, and Beacon Hill is flinty slate†.—† Rep. i. 18 and 19.

l. 23 and 24, gradations from sienite‡.—‡ Rep. i. 153 and 154.

291, l. 3, whetstone or hone*.—* On Whittle Hill, Rep. i. 61 and 439, see my Note on p. 171.

l. 4, Markfield Knowl†. — † Markfield Windmill-hill, Rep. i. 45 and 144.

292, l. 1, deep perpendicular fissures*.—* Occasioned by the shrinking of the thick beds or masses, in unequal degrees, as is often to be observed in Cliffs and Quarries.

l. 16, concealed by plantations and inclosures†.—† The many openings and excavations which will be made in the surface, by the formation of the Roads, and digging of Stone

[P. 292] Stone for them, and the Walls, searching for *Slate*, (Rep. i. p. 154, at bottom), &c. &c. will greatly increase, instead of diminishing, the means of observation and of understanding the internal structure of this curious district.

293, l. 17 and 18, covered with Limestone*. — * On their western side only?, P. M. xl. p. 54.

294, l. 18, natural history of the island*.—* In August 1807, the materials were begun to be collected, on a comprehensive plan, for a Mineral Map and History of *Derbyshire and its Environs*, and the field work thereof was persevered in, until November 1809: an *abstract* of all the most generally useful parts of which, were purchased by the Board of Agriculture (Derby Rep. i. p. v. and vi.), and 150*l.* paid to me for the same, on the 29th of April 1810. In the following Month, Mr. Arthur Aikin¶ issued a Prospectus of the *Shropshire Survey* here alluded to by Mr. B., the field business of which is yet in hand, I believe.

On the 12th of June I waited on the Board, by desire, and, *as I had before done*, produced and explained my large and reduced Mineral Maps, Sections, &c.¶¶ and a part of the MS. of the 1st vol. of my Derby Report, (since published by the Board in June 1811); soon after I retired, the Session of the Board for the year 1810 was closed, by a long *Speech* from the President Sir John Sinclair, which was soon after printed and circulated. In this Speech, a general review was, as usual, taken, of the proceedings of the Board, as to the commencement, progress or conclusion, of any part of the labours intrusted to them by the Government and the Country; and herein it was stated, that the Board had recently *patronized* the *Survey of Shropshire* by Mr. Aikin, concluding thus, "and which is *the first attempt of the sort*, on a regular and extensive plan;" and yet, not a mention or

¶ One of the Council and *Secretaries* of the Geological Society, see a Note in my 1st Letter, vol. xlii. p. 56.

¶¶ Mr. *William Smith* has in like manner, on different occasions, attended meetings of the Board of Agriculture, and submitted and explained his Mineral Maps and Sections, of great part of England, as I have mentioned or hinted, P. M. vol. xxxix. p. 426, and xxxv. p. 114. In Mr. S. and my own cases, the matters thus *submitted*, in hopes of obtaining the efficient *patronage*, for lack of which, they are *yet unavailable by the Public*, (P. M. xlii. p. 109 and 246) were not merely, proposals and *promises* of field-business, *intended to be undertaken*, and of Maps, &c. *to be prepared* therefrom: but *the large Maps, Sections, &c. themselves*, which had been the results of unwearied researches for years, and of expenses to the parties, almost beyond their private means.—Mr. A's patronage, and Geognostic connections, will, it is hoped, produce to him a very different and more agreeable result.

most

[P.294] most distant allusion was made, to my *Derbyshire Mineral Survey*, or, as to the Board having contracted for the Manuscript of *the first Mineral Report on the English Counties* (which series had been promised ever since 1793, Rep. i. pref. vi. and P. M. xxxvii. p. 8) in this Speech, or in any other announcement from the Officers of the Board to its Members or to the Public, as far as I have heard:—I forbear further comments.

295, l. 22, similar to the limestone*.—* And supposed to be *the same*, in pages 220 and 274, see my Note on the latter page.

l. 24, eastern side of North Wales†.—† It is not on the eastern side only, of the northern Cambrian Mountains, that Limestone Rocks underlay Coal-measures, but on the NE and NW sides also; the latter calcareous Rocks, stretching across Anglesea, see my Note on page 275.

296, l. 18, said to occur in Anglesea*.—* During my stay in this Island, I saw, or could hear accounts from the inhabitants of no Rocks of Granite: the Mile-stones on the Post Road, seemed formed of reddish Granite, and occasional loose blocks of it were met with on the surface: but Mr. Wilson Lowry has since informed me, that in travelling across from the Post Road at Gwndy to the Paris Mountain, he saw low Rocks of Granite, not far from the former place.

l. 22, from its hardness, beauty†.—† See my Note on p. 92. Very considerable Rocks of Serpentine are found in the coarse Slate of the NW and W parts of Anglesea.

297, l. 9 and 10, sulphurous limestone*.—* See my Note on p. 98.

299, l. 20, boundary of the coal strata*.—* See p. 268. A similar "concavity" or *trough* of the same Limestone, seems to underlie the estuary of the Dee, and the Coal-measures therein, seem to extend from Flint to Park-Gate: how far this Coal-basin extends westward, under the Sea, towards the north Coast of Anglesea, &c. we have yet in part to learn, see my Notes on p. 108, 275, and 285.

300, l. 10, part of Somersetshire*.—* But a very small part of this County, the Quantook Hills, at its western extremity, can I think be either "primary" or "transition;" unless that Lias, Red Marl, and Coal-measures *under these*, are so? see my Note on pages 12 and 283. Mr. Thomas Allan says in the Edin. Trans., that in his way to Exeter, he first saw "transition strata between Bridgewater and Taunton:" but on reference to the Appendix to his paper, No. 1 and 2, it appears, I think, that the imbedded Grit-stone in the Red

[P.300] Red Marl, something harder than usual perhaps, and tilted, has been so denominated; for, near to Taunton, on the SE, the *Lias* occurs again, as I have been informed.

302, l. 11, and worn down *.—* See Mr. John Hutchinson's Works xii. p. 261 and 338.

303, l. 3 and 4, as a field bean*.—* Some of the pieces of Gold from the Wicklow Mountains are much larger, appearing as if kneaded into their present form, by violent pressure (if not aggregated by this means), and containing some fragments of quartz, which appear forced into the gold.

304, l. 2, and warm springs*.—* Should not cold or ordinary *Springs* also have been noticed in Mr. B's work?, and their undoubted sources from *the infiltration of rain waters* been particularly mentioned: few of the Geological phænomena are of more general interest, or have been more superficially, or erroneously treated by Writers; see Rep. i. 500.

l. 10, under the whole district †.—† This is unsupported by any facts or observations in the Mines, &c. Rep. i. 487.

l. 16 and 17, the *Cromford* Canal ‡.—‡ The cause of the temperature of the western end of this Canal being often higher than usual, is perfectly apparent: it is fed by the large stream of very *warm water issuing from Cromford Sough*, (Rep. i. 329, and vol. iii. chap. xvi. sect. 3), on Sundays and often in the night, when Mr. Arkwright's Cotton Mill is not at work.

l. 19, be frequently seen rising**.—** Since Mr. B. told me in May 1812, of this supposed prevalence of *warm springs and general heat in the strata*, through which the *Cromford* Canal is cut, from Cromford to near Crich, I have made very particular inquiries, while in the neighbourhood, and concerning the Person, formerly of Crich, whom he mentioned as his informant. The hot spring which rose at Middleton-Wood Bath, (Rep. i. 505), in Bonsal Dale, close by the side of the great Limestone Fault, (Rep. i. bottom of p. 281 and in p. 282), until that by the driving of the Cromford *Sough*, a porous stratum of Limestone, or a line of fissures therein, connected with it, let off its hot water, at a much lower level, and mixed it with a great deal of cold water, which the sough still collects, on both sides of that which enters it hot; this mixed water, almost daily let into the Canal at its upper end, will perfectly account for all the volcanic wonders, which are here accumulated by Mr. B.; see my Note on p. 226.

305, l. 1, principally confined to Basaltic*.—* Where are the

Basaltic

[P.305] Basaltic strata, or the volcanic substances, near to Bath andBristol ?: whose Springs seem to me to rise, from some certain stratum in or near the Lias Limestone; as a line of Mineral Springs also do, at Cheltenham, (and several places in its neighbourhood, P. M. xxxii. p. 59), Lemington-Priors, Kings Newnham (Warwick. Rep. p. 28), and other places, across the greater part of England.

l. 10, situation of the warm springs †.—† These in Derbyshire, rise by the sides of the great Faults, see Rep. i. 503, 504, 505, and my Notes on p. 226 and 304.

306, l. 20, that clearly indicate*.—* Hot springs, and strongly mineralized ones, more clearly indicate the existence of Faults, and the chemical and perhaps the Voltaic action of strata *on each other*, than point out dormant Volcanoes, see my Notes on p. 226 and 304.

309, l. 20, never will be accomplished*.—* Why not?—If Mr. B. could have found as many materials *prepared to his hands*, respecting several other English Counties, as he has with respect to one, he might have given us a work, far more distinguished by variety, and truth of representation.

l. 25, is what ignorance and chance †.—† Did these alone, produce the "Mineralogia Cornubiensis" of W. Pryce, "British Mineralogy," by J. Sowerby, "The Mineralogy of Derbyshire," by J. Mawe, &c. &c.? or did they (ignorance and chance) set on foot, the Surveys of Dumfriesshire, Shropshire and Derbyshire? To say nothing of Mr. Smith.—In *an Advertisement*, such assertions as these may pass current, but ought to have been more cautiously introduced into an elementary work on the Science.

310, l. 10 and 11, the year 1804*.—* This circumstance, so perpetually referred to by Mr. B. in his writings, happens to be rather incorrect, see my Note on p. 221.

311, l. 24, resemblance to the Welsh limestone*.—* The pozolanic quality of the *Blue Lias*, near Bath in particular, (where the name originated) has been *well known for ages*, and particularly so since 1791, when Mr. Smeaton made it so extensively known, by his publication on the Eddystone Light-House, and since 1794 or 5, when Mr. Smith began to exhibit at Mitford and Bath, &c. his Maps showing the range across England of this very important stratum, see Rep. i. 114, and P. M. xxxvi. p. 105: its use in Coalmines, I have mentioned, Rep. i. 327.

314, l. 1 and 2, its effects on a large scale*.—* Rep. i. 153. Phil. Trans. 1811, and P. M. xxxvii. p. 441, and xxxix. p. 29 and 426.

[P.314] l. 24, if *slowly* cooled†.—† At the Iron Furnace (and Coal-works) to which Mr. B. alludes in p. 282, Mr. David Mushet observed, that the Slag let out of the Furnace and *quickly* cooled by throwing water on it, had often, perfect short octagonal prisms of whitish slag, found in cavities within it, and some small transparent cubic crystals, &c.

315, l. 7, shrinking in of the basalt* —* The crystallization of Basalt on the large scale, is so imperfect, before exposure *on or near to the surface,* that it is found a solid and homogeneous Rock (except as to imbedded masses), whenever sunk or driven to, in the deep, in Mines, and the amygdaloidal holes are then all *full* of spar, &c. Rep. i. 277, see my Note on p. 123, and P. M. xliii. p. 136.

317, l. 4, is owing to the mica*.—* Rep. i. 428 and 466.

l. 9, dividing in a contrary direction†.—† A phænomenon regarding stratification, which I have called *stratula,* Rep. i. 155, is here alluded to, and it rather surprises me, that Mr. B. has not in his work, taken more notice of the fact, that in many Coal-measure and other Rocks, of Gritstone in particular, certain beds, are uniformly crossed at a high angle by inferior stratification; these stratula ending with an oblique or bevelled end at top and bottom: in different beds in the same quarry, the *stratula* will sometimes be found inclined at different angles, and sometimes in contrary directions, from those in other Beds. Writers seem to have been little acquainted with this phænomenon, (Mont. Mag. xxxiii. p. 517, P. M. xxxviii. p. 357, &c.) and many erroneous descriptions of *the dips* of strata have been given in consequence. Mr. Townsend supposed, when he saw the stratula of a thick bed of Rock near Bath, that an enormous convulsion had happened, by which the strata were first tilted almost to an angle of 46°, and then their *top* edges (and he tells us nothing of their *bottom* edges) were swept or worn off to an exact plane, and that after this, other strata were horizontally deposited upon them! Such, I am inclined to think, have in many instances been, the "unconformable" masses, described by the Anglo-Wernerians, see my Note on p. 45.

318, l. 9, was in a solid state*.—* As happens very frequently to Gritstones, with argillaceous cements, which are burnt for the Roads, (Rep. i. 164, and iii. 256), and thereby assume a pretty regular columnar structure.

325, l. 22, great ecliptic days*.—* But why need these grand events be *measured by days* of any kind?. The Mosaic account, here obliquely glanced at, does not require it; if all the

[P.325] the stratified Animals and Vegetables, successively existed and became extinct, as the strata were *successively Created*, before the periods called *days* (but improperly so in our translation) commenced, and to which Creation of matter and of the now extinct organized races, only the very first words of Moses apply, see the articles *Coal* and *Colliery* in Dr. Rees's Cyclopædia.

327, l. 6, *in* the upper strata*.—* In alluvium *upon* the strata, see my Notes on pages 16 and 181.

328, l. 15, astronomy for the discovery*.—* Perhaps a more extended and minute application of Gravity and tidal action, than has yet been made, will solve many difficulties, as to the changes that the Earth has undergone, P. M. xxviii p. 128 and xxxix. p. 358 note, and my Note on p. 192.

329, l. 8, concretion of stony masses*.—* Is it not far more probable, that the *meteoric stones* have existed through many ages as *satellitulæ* of our Planet, and visit our Atmosphere at very short intervals, when in Perigeo, as *Shooting Stars*, see Mr. Nicholson's Journal, vol. xxxiv. p. 298: the *hypothesis* that makes each one of them a separate volcanic Bomb from the Moon, seems a very lame and unphilosophical one.

335, l. 4 and 5, succession of different soils*.—* See P. M. xxxv. p. 116.

l. 6, extraordinary organic remains †.—† P. M. xxxv. p. 124.

l. 28, of fresh-water shells ‡.—‡ Query?—see my Note on page 160. Mr. *William Smith* lately informed me, that in driving piles for a drainage Sluice on the sea shore at Minsmer S of Dunwich in Suffolk, the bottoms of them, below the Sand, entered a soft limestone Rock, which he stated, may very probably be part of the same stratum as that at West Cowes on the northern coast of the Isle of Wight, that has been supposed to contain the *fresh-water* shells, to which Mr. B. here alludes.

336, l. 6, in the following order*.—* See P. M. xxxv. p. 140.

l. 13 and 14, and Gypsum †.—† See my Note on page 173.—The Paris Gypsum, &c. seems to have been traced through France, to the confines of Switzerland; and isolated hummocks of it to appear, far north-east of the Paris Basin, (as it is called) at Luneberg in Hanover, and at Segeberg and Kiel in Holstein, according to Dr. Steffens and M. De Luc. At Luneberg and at Oldersboke near Segeberg, strong Salt Springs arise, from this Gypsum stratum, I believe.

 l. 24,

[P. 336] l. 24, black earth ‡.—‡ Bones of Elephants, Oxen, Antelopes, &c. and large Trees, &c. are found *in this most recent alluvium*, with rounded Flints, &c. P. M. xxxv. p. 58, see my Note on page 181.

P. 343, l. 11, composed chiefly of gravel*.—* Rep. i. 252.

344, l. 28, with a *floor**.—* A rib or skirt, of Cawk, &c.

350, l. 15, indurated clay*.—* Bind is indurated *Loam*, and Clunch is indurated *Clay*, see p. 351, and Rep. i. 445 and 446.

l. 23, composed of *rounded* stones†.—†*Breccias* contain angular stones or rubble; *Pudding-stones* contain round nodules; and *Gravel Rock* contains rounded stones. Rep. i. 142.

351, l. 8, sulphate of barytes*.—* See Rep. i. 355 N.

352, l. 15, Dip, the point of the compass*.—* The most common and proper application of this term, when used alone, is to the *degree* of descent or inclination, which practical Men express in numbers thus, 1 in 5, and authors for the same write, 11° 32′, &c. The point of the compass or *direction* of the dip is necessary also, as SE or E 45° S.

361, l. 22 and 23, upon the above plan *is new**.—* Mr. Wm. Smith, Mr. John Farey, Sen. Mr. Thomas Bartley, (formerly a Clerk and Assistant to Mr. Smith), &c. did the same, for various Noblemen and Gentlemen, long before Mr. B.

Postscript.—I beg to mention, before I close these *Notes*, that it appears by a Letter of the 29th of April 1814, from Mr. *Elias Hall*, of Castleton, to Sir Joseph Banks, Bart., which he did me the honour to show me, that in the present Spring Mr. Hall has extended his survey northward from the Peak of Derbyshire, and made a *Model*, (see P. M. xlii. p. 113) of the *Strata* of the *Grand Ridge*, and the adjacent country east and west of it, almost as far as Westmorland; and that he finds Pennigant Hill, N of Settle in Yorkshire, to be capped with the 2d Grit Rock, and to have a basis of 1st Limestone.

More than ten years ago, I learned from Mr. Wm. Smith, that Whernside hill, about 6½ miles NW of Pennigant, was capped *with Coal-measures* (although esteemed there, to be the highest hill in England); and this latter circumstance it was, as well as the want of evidence offered by Mr. B., either in his Lectures or his Geological Work, of his favourite position; viz. that the 4th Limestone shewed itself in the lower parts of these and Ingleborough Hills, and *Slate from under this lowest of the Derbyshire Peak Limestones*, which made me doubt the correctness

rectness of Mr. B's observations, and occasioned me to resist his conclusions, as I have done in my three Letters and *Notes* on Mr. B's Work, in this and the previous volume of the Phil. Mag.

Mr. Hall's observations seem to me to render it now almost certain, that Mr. B. has mistaken the 1st Limestone Rock for the 4th, as I have often before hinted, and the result seems an important one; viz. that the *Slate* of Ingleborough (see vol. xlii. p. 59 and 170) must probably be referred to *the same stratum* as the 1st *Toadstone* of Derbyshire! instead of a stratum under the 4th Limestone, as Mr. B. has contended. How far this new suggestion, of which I have recently added a hint, in my Note on page 275, may be correct, and its influence may extend towards the explanation of the real structure of the British Isles, I shall be industrious to inquire, and shall gladly receive the facts or suggestions that your Correspondents may be able to offer: the means for which will now I trust be greatly extended, by the speedy publication of *Mr. Smith's large Map of the Strata*, which may now be seen, in part, and Prospectus obtained, by application to Mr. John Cary, No. 181, Strand.

I am, sir, your obedient servant,

12, Upper Crown Street, Westminster. JOHN FAREY, Sen.

To Messrs. Nicholson and Tilloch.

LXXII. *Facts and Observations towards a History of the Combinations of the yellow Oxide of Lead with the Nitric and Nitrous Acids.* By M. CHEVREUL.

[Continued from p. 270.]

Analysis of the Nitrites of Lead.

30. THE nitrite of lead, when exposed to the sun for several days, retains some water, as we may see on heating it in a long close glass tube: that which has been exposed in a retort at the heat of boiling water, until no more humidity is extricated, loses a portion of acid.

100 parts of nitrite which had been exposed to the sun, lost by heat from 19·5 to 20 of acid and water.

31. As the acid of the nitrite begins to be extricated from it at the temperature which is necessary to separate the water from it, I preferred analysing a nitrite which could retain water, rather than trouble myself to analyse one which might have lost acid: I think therefore that the salt which was used in the following analysis retained a little water. It had been dried in the sand-bath with great care. It yielded

Acid	18·15	100
Oxide	81·85	450

 As

As it may reasonably be supposed that a little acid might have been volatilized when the nitrite was dried, I distilled a certain quantity of this salt dried in the sun, in a small retort communicating with a tube filled with muriate of lime: the result was conformable to that of the preceding analysis: deducting the water only, the quantity of base was a little more than in this analysis.

32. 100 parts of subnitrite which had been exposed to the sun, and at a temperature of 100° (centigrade), lost by heat 10·5 of acid and water. The same salt dried in the sand-bath with great care yielded

Acid	9·9	100
Oxide	90·1	910

33. The analysis of nitrites of lead proves that in the subnitrite the base is double that of the nitrite; for, if we multiply by two 450, which is the quantity of oxide found in the nitrite, we have 900: and in the analysis of the subnitrite we found 910.

34. In the sulphites and alkaline nitrites we know that the base is in the same proportion with the radical of the acid, as in the sulphates and the nitrates of the same bases; so that if we take away, for example, from the sulphate and nitrate of potash, the quantity of oxygen which forms the difference between the sulphur*ic* and the sulphur*ous* acid, and between the nitr*ic* and the nitr*ous* acid, we ought to have sulphite and nitrite at the same degree of saturation with the salts from which they proceed. The same appears to be the case with the nitrate and nitrite of lead. We may judge of this if we convert the acid of the nitrate into nitrous acid. According to M. G. Lussac, 100 parts of nitric acid contained 88·203 of nitrous acid. If what we have said be exact, the nitrite ought to be formed of

Acid	88·203	17·85
Oxide	406·000	82·15
	494·203	100·000

Now if we compare this proportion with that found by analysis, we shall see that the difference is only 30 centiemes of one part in a hundred.

35. I wished to know if there was a nitrite of lead corresponding to the acid nitrate; and I thought that, if this salt existed, I ought to find it in the solution of nitrites precipitated by the carbonic acid; for I have said a little higher, that this liquor contained oxide of lead and nitrous acid in excess: I calculated in the first place the proportion, and I found that it must be formed, supposing that it existed, of

Acid	88·203	30·3
Oxide	203·000	69·7
	291·203	100·0

36. I

36. I dissolved in water five grammes of subnitrite of lead, containing

Acid	0·4917
Base	4·4750
Water	0·0333
	5·0000

I passed a current of carbonic acid into the solution, and I separated 3·18 gr. of oxide in the state of carbonate*: there remained therefore in the liquor 1·2950 gr. of oxide, and 0·4917 gr. of acid, or

Acid	27·52
Base	72·48

Which differs by 2·78 from the determination made by calculation.

37. The solution precipitated by the carbonic acid was concentrated, and there was an extrication of nitrous acid; and upon cooling, yellow leafy crystals of nitrite were deposited. The mother-water several times concentrated gave leafy nitrite to the last†.

38. In order to determine what was the decomposition which the nitrite of lead would undergo by the carbonic acid, I took five grammes of nitrite (coming from the subnitrite) which contained

Acid	0·887
Oxide	4·006
Water	0·113
	5·000

The carbonic acid precipitated 1·74 gr. of oxide. There remained therefore in the liquor 2·260 gr. of base and 0·887 of acid, or

Acid	28·19
Oxide	71·81
	100·00

which

* I made two analyses of carbonate of lead coming from the nitrite decomposed by the carbonic acid gas, and obtained

Acid	16·36	16·35
Base	83·64	83·65

M. Berzelius found

Acid	16·50
Base	83·50

† I set aside a portion of this mother-water, with a view to ascertain if the crystals which it should deposit by a very slow evaporation, would not contain more acid than those obtained by concentration and cooling. I obtained in this way yellow crystals formed of leaves united in

which differs by 2·11 from the determination made by the calculation*. The solution of nitrite separated from the carbonate, and afterwards concentrated, acted like that of the subnitrite; it gave out nitrous acid, and yielded crystals of nitrite.

39. It follows from these facts: 1st, that the nitrite and the subnitrite are partly decomposed by the carbonic acid, because the affinity and the quantity of the nitrous acid are insufficient to overcome the whole of the tendency which the carbonic acid has to form an insoluble combination with the oxide of lead; but the instant there is a certain quantity of nitrous acid set free the decomposition stops, because this acid is much more soluble than the carbonic: 2dly, that the nitrous acid of a solution of nitrite, when passed to the carbonic acid, seems to be to the oxide which is not precipitated from it in a proportion corresponding to that of the elements of the acid nitrate of lead: 3dly, that the affinity of the nitrous acid set free for the nitrite is not great, since by concentration and crystallization we obtain nitrite only: 4thly, that, the nitrous acid set free being very capable of separation from the nitrite, it is possible to decompose a given quantity of nitrite by the carbonic acid.

40. I shall conclude this memoir with an examination of the nitrite of lead prepared by M. Proust's process. I have said a little higher, that when we wished to prepare this salt, and when we boiled too long the acid nitrate of lead over this metal, we obtained a nitrite which contained subnitrite, or more base than the nitrite†. In order to avoid the production of the subnitrite, I made several experiments with a view to ascertain if each of them would give me a homogeneous and identical product. I obtained two varieties of yellow crystals, one in compact leaves, the other in flexible scales. These crystals being of the same species, I confounded them together. I regarded them

in stars. I thought at first that they were acid, because they reddened turnsole paper; but, having pressed and dried them, they gave a solution which reddened at first turnsole paper slightly, but soon became blue again. I had not enough of these crystals to ascertain by analysis their quantity of base. The solution yielded yellow needles.

* In an experiment similar to the latter, I obtained

Acid	30·7
Base	69·3

which is ostensibly the same with that of the calculation: but what prevents me from having as much confidence in it as in the foregoing, is, that the nitrite employed had been crystallized twice, and that it seemed to contain a little subnitrite, or more base than the nitrite.

† This salt is of a redder colour than the nitrite. When we dissolve it in boiling water, we obtain yellow and orange crystals, which contain much more base than the nitrite, and less than the subnitrite.

100 parts of this nitrite contained

Acid and water	16·56
Base	83·44

at

at first as pure nitrite; and this appeared so much the more probable, as they presented to the eye all the characters of a homogeneous substance, and as they were formed in the midst of a great mass of liquid; but ulterior experiments convinced me that this salt was formed of nitrite and nitrate, and that it was a kind of double salt with two acids. I was led to this singular result by the decomposition which the carbonic acid made it undergo.

41. In order to show more evidently the difference of these crystals from the nitrite coming from the subnitrite, I shall suppose them formed of Nitrous acid .... 0·887 gr.

Oxide	4·000*
Water	0·113
	5·000

I passed into the solution of these five grammes a stream of carbonic acid, and I separated from it 2·20 gr. of oxide: there remained therefore in the liquor 1·80 gr. of base and 0·887 of acid: this report gives for 100,

Acid	33·02
Base	66·98

In this experiment the salt had been dissolved in nine decilitres of water. In another I put the same salt in powder in three decilitres of water, and obtained by the carbonic acid 2·21 gr. of base. (I had made this experiment with a view to ascertain if the quantity of water had any influence on the quantity of oxide precipitated.) A third and fourth experiment gave me 2·20 gr. and 2·23. They concur therefore perfectly in proving that the carbonic acid separates more base from the nitrite prepared with the acid nitrate of lead and the metal than from that obtained from the subnitrite.

42. The solution precipitated by the carbonic acid evaporated, gave off nitrous acid, and deposited upon cooling *yellowish white scales:* the mother-water of these scales, concentrated, gave *small white needles;* and the mother-water of these needles, spontaneously evaporated, *yellow crystals resembling by their form the acid nitrate of lead.* Now as the nitrite coming from the subnitrite only gives, when we decompose it by the carbonic acid, yellow nitrite and nitrous acid, and not white salt, we must conclude that the nitrite prepared by M. Proust's process differs from it in its composition.

43. I examined in the first place the white needles, and after having crystallized them several times I found them similar

* The proportion of the base had been determined by experiment, and the water and the acid had been calculated from the analysis of the nitrite (31).

to

to the nitrate of lead *; for they did not colour water; they did not give out nitrous acid when boiled in nitric acid: they did not give nitrite or subnitrite when boiled in water with oxide of lead; they were reduced by the carbonic acid into carbonate and into acid nitrate of lead.

44. *The yellowish white scales* redissolved in boiling water gave *yellow leaves*, and a mother-water not highly coloured, which gave white needles of nitrate of lead mixed with a little nitrite. The yellow leaves still retained some nitrate. It was evident that these scales were a combination of nitrite and nitrate of lead.

45. *The yellow crystals resembling in form the acid nitrate of lead*, redissolved in boiling water, deposited small needles and small leaves of nitrate of lead; the mother-water which remained was acid and slightly coloured. The composition of these crystals is analogous therefore to that of the scales, but they differ in so far as they contain more nitrate: it is not improbable that they contain acid nitrate: the nitrate of lead which they yield when dissolved in boiling water does not contradict this idea; for, if it be true that the acid nitrate of lead when boiled with the nitrite decomposes it, and passes to the state of nitrate, this is not a sufficient reason for thinking that this decomposition takes place in a liquor left to itself, and which would contain oxide of lead with an excess of nitrous and nitric acid.

46. The decomposition of the *scales* and of the *yellowish crystals* effected by the affinity of water, and the force of cohesion, led me to examine the action of the water on the nitrite of lead prepared by M. Proust's process. On dissolving this salt in boiling water, I obtained, 1st, yellow leafy crystals which were deposited upon cooling; 2dly, crystals slightly yellow: they were deposited by concentration and cooling from the mother-water of the former crystals; 3dly, a slightly coloured mother-water which yielded small yellow crystals, and small white grainy crystals. On passing a stream of carbonic acid into this mother-water, I obtained plenty of nitrate from it.

47. I am of opinion that these facts ought to leave no doubt as to the compound nature of the nitrite of lead prepared after M. Proust's method. It is on this account, in the experiments which I made upon this salt, that I employed the nitrite coming from

* In my comparative experiments with the pure nitrite, and that of M. Proust, I am convinced that the nitrate obtained from the latter was not formed in the course of the operation. It is nevertheless not impossible that it may be formed under certain circumstances; for we can conceive that the nitrous acid, which is extricated when we evaporate the acid nitrate of lead, may, by combining with steam and oxygen, form nitric acid, a portion of which may be mixed with the nitrite, and decompose it.

from the subnitrite. This result naturally leads to this question: Is there a term at which the acid nitrate of lead boiled over this metal is converted into simple nitrite, so that by stopping the operation at this term we may obtain pure nitrite? I do not think that there are as yet a sufficient number of facts to resolve this question definitively: but all my experiments have yielded pure nitrite; and I have observed besides, that when there was an evident production of subnitrite, nitrate was still found in the liquor. Another fact is, that nitrites, prepared in different operations decomposed by the carbonic acid, have given the same quantity of carbonate; which seems to indicate a constant composition.

Recapitulation.

1. The oxide of lead boiled with the acid nitrate forms a salt, the base of which is double that of the acid nitrate.

2. When we boil lead with acid nitrate, the metal is oxidated at the expense of the nitric acid, and passes to the state of litharge: the latter is united to nitrous acid. In this operation, therefore, there is a nitrate formed with a base of an oxide more at the minimum than litharge.

3. The combination of oxide of lead with the nitric acid is not the only salt of its kind which is converted into nitrite by lead; the nitrate of potash undergoes a similar decomposition.

4. The nitrous acid gives with oxide of lead two combinations: the one, which is a subnitrite, is formed when we boil the acid nitrate of lead over this metal until there is no longer any action; the other, which is the nitrite, is obtained by passing a current of carbonic acid into the solution of the subnitrite.

5. The colour of the subnitrite more easily disappears than that of the nitrite, for the former does not colour the water like the latter. The proof that non-colouring of the water by the subnitrite is not owing to the salt being less soluble than the nitrite, is, that by precipitating from its solution a part of its oxide, the liquor becomes yellow.

6. The solution of the two nitrites precipitates the nitrate of copper; the precipitate is formed of two metallic hydrates, which probably retain a little nitric acid.

7. The nitric acid and acetic acid, when boiling, emit nitrous vapour when we project into them the nitrites reduced into powder.

8. The conversion of the nitrite into subnitrite by the oxide of lead, is very proper to prove that, in the preparation of the nitrite by M. Proust's process, there is not formed any oxide more at the minimum than litharge; for, if this were the case, instead of an oxide inferior to litharge, it must be acknowledged that there were two; since I have demonstrated that, by prolonging the ebullition of the acid nitrate of lead over the metal, we obtain

tain a salt quite different from M. Proust's, insomuch as it contains more lead. Now this opinion being admitted, it would be no longer possible to explain how M. Proust's yellow salt could be converted by litharge into a salt the base of which was less oxidated than his.

9. The combinations of the oxide of lead with the nitrous acid confirm the laws established by Richter, Gay-Lussac, Wollaston and Berzelius: thus the quantity of base of the subnitrite is double that of the nitrite. The composition of the nitrite corresponds with that of the nitrate of lead; and a remarkable result, if it be not accidental, is, that the carbonic acid, by taking from the nitrites a portion of base, leaves in the liquor a quantity of oxide, which is to that of the nitrous acid in a proportion which seems to correspond with that of the elements of the acid nitrate. But the cohesion of the nitrite and the expansive force of the acid are sufficient to overcome the affinity of the nitrite for an excess of its acid; so that, by concentrating by heat the solution of the nitrites when passed to the carbonic acid, we obtain nitrite by cooling*. It would be curious to ascertain if, in the decompositions analogous to that of the nitrite, something similar does not take place in the proportion of the principle which has become predominant. This would be the means of ascertaining if the result which I have observed is not accidental. It would be also interesting to ascertain if the alkalis, in acting on the acid nitrate of lead, do not produce a subnitrate corresponding to the subnitrate.

10. I have mentioned several facts in this memoir, which prove that we obtain nitrites intermediary between the two combinations which I have described. May we regard these salts as so many different kinds of nitrite, or as combinations of the two nitrites? This is precisely what I cannot decide in a definitive manner: I shall therefore leave to my readers the liberty of forming any opinion they may think most natural.

11. I have not been able to obtain pure nitrite by M. Proust's process: for, if I do not deceive myself, the salts which I have prepared by this process were a combination of nitrite and nitrate: this is what I have endeavoured by examining the action which they experience from carbonic acid and water. Although I was not able to make pure nitrite by boiling the acid nitrate of lead over this metal, I have not affirmed that it is impossible to do so; because the want of success in operations of this kind is frequently owing to circumstances which it is not always easy to ascertain.

* It is not improbable that by counteracting the causes which tend to prevent the combination of the nitrite of lead with an excess of its acid, we may succeed in obtaining this combination in the solid state.

LXXIII. *New*

LXXIII. *New Outlines of Chemical Philosophy.* By EZ. WALKER, Esq. *of Lynn, Norfolk.*

[Continued from p. 285.]

On Light and Heat.

SIRS, LAVOISIER's theory of combustion is founded on the supposition, that in every case oxygen combines with the burning body, and that the weight of the product of every body after combustion corresponds with the weight of the body before combustion, *plus* that of the oxygen consumed.

But this theory is questionable, for no account is taken of the light and heat generated in the process. If a long mould candle, which weighs two ounces troy, be lighted, and placed to form an angle with the horizon of 60 degrees, it will be so perfectly consumed as not to leave a single grain of ashes; the only product being pure water.

I have observed in a former paper, that oxygen gas is composed of thermogen and water*, so that, when thermogen combines with the burning body, the base of the gas being incombustible becomes the product.

Now, the important problem to be investigated is, what becomes of the *two ounces* of matter of the candle? Is it converted into light and heat, or is it transmuted into pure water? Now, unless it can be demonstrated that tallow, oil, and wax are transmutable into pure water, the theory of combustion of M. Lavoisier falls to the ground.

It is well known that a charged Leyden jar contains nothing that is either tangible, visible, or ponderable; but let it be discharged through the air, or some other conducting substance, and both light and heat will be generated; nor can either light or heat be produced, unless the two elements of combustion be united to ponderable matter. Consequently, *light* is a *triple compound* of thermogen, photogen, and combustible matter; and *heat* is a *compound* of the same two elements united to matter of the same kind, but differently modified.

As light comes from the sun in seven seconds and a half, it, therefore, passes over five miles in about $\frac{1}{50000}$th† part of a second of time. A candle which weighs two ounces troy will burn about ten hours, consequently $\frac{1}{200}$th‡ part of a grain will

* Page 24 of this volume.

† As $\overset{\text{Miles.}}{95{,}000{,}000} : 7'\ 30''\ (450'') :: \overset{\text{Miles.}}{5} : \frac{450 \times 5}{95{,}000{,}000} = \cdot 0000''2 + = \frac{1}{50000}$ of a second.

‡ As $\overset{\text{H.}}{10} : \overset{\text{Oz.}}{2} :: \frac{1}{50000}'' : \frac{1}{200}$ of a grain.

be

be consumed in $\frac{1}{50000}$th of a second: and as a candle may be seen in a dark night, the air being clear, at the distance of five miles, a sphere of ten miles in diameter, containing 523 cubic miles, is filled with light in $\frac{1}{50000}$th part of a second.

But as the heat of a candle cannot produce any sensible effect at a greater distance than a few yards, we may suppose that heat is specifically heavier than light. Oxygen gas is heavier than hydrogen gas in the ratio of 15 to 1, because the former contains more water than the latter. Let it, therefore, be supposed that the quantities of matter contained in light and heat be in the ratio of 1 to 15, which appears to be considerably within the bounds of probability, then a quantity of matter of about $\frac{1}{3000}$th* part of a grain may be so attenuated and modified by the elements of combustion, thermogen and photogen, as to fill a space $=523$ cubic miles in $\frac{1}{50000}$th part of a second of time.

Hence we see that, although matter is passive and inert, yet, by being acted upon by the invisible imponderable elements, it is made to put on an infinite variety of new forms, without a single atom being lost. And the wonderful effects which are thus produced on the living functions of animals, and vegetables, is a field of investigation that is almost boundless.

Lynn, May 12, 1814. E. WALKER.

[To be continued.]

LXXIV. *Process for preparing, with a Description of some of the Properties of, the refined Ox Gall, invented and prepared by* PELTRO WILLIAM TOMKINS, *Esq. Historical Engraver to Her Majesty, No.* 53, *New Bond-Street, London* †.

OX GALL deprived of its tendency to putridity, and its colouring matter, so detrimental to all delicate colours, is prepared in the following manner:

Process.

To a pint of fresh ox gall, boiled and skimmed, put one ounce of alum, finely powdered; continue it on the fire until combined; when cold, put it into a bottle, and cork it moderately close.

To another pint of fresh ox gall, also boiled and skimmed,

* $\frac{1}{15}$ of $\frac{1}{200} = \frac{1}{3000}$.

† The above is accompanied in the *Transactions of the Society of Arts* for 1813, whence we have copied it, with numerous certificates from our first artists, all concurring in the valuable properties of the refined ox gall.— The lesser gold medal of the Society was voted to Mr. Tomkins for his communication.

put

put one ounce of common salt, and continue it on the fire until combined; when cold, put it into another bottle, and cork it moderately close.

Gall, thus prepared, will keep perfectly free from putridity, or any offensive smell, for years.

When the above preparations have stood in a room, of a moderate temperature, for about three months, they will deposit a thick sediment, become clear, and fit for use in ordinary purposes; but as they contain a large portion of yellow colouring matter, tingeing blue of a greenish hue, reds, brown, and sullying purples, they are unfit for general use in painting in water colours.

Further Process.

The before-mentioned preparations, after standing until become perfectly clear, are to be decanted and combined in equal proportions; a thick coagulum is instantly formed of the yellow colouring matter, which precipitates, leaving a clear liquid, namely, the colourless ox gall.

N. B. After the combination of the two first preparations, the process may be assisted by filtering the liquid through paper. Age renders this preparation more brilliantly clear, and by it, it seems to obtain an agreeable scent; nor has it been observed to contract, at any time, an unpleasant smell, or to lose its useful properties.

Properties.

The refined ox gall possesses all the valuable properties of ox gall, as applicable to painting in water colours, with the superior advantages of being deprived of all tendency to putridity, and of all colouring matters.

It combines with, and fixes, all water colours, as they are usually prepared, either by being mixed with them, or washed over them after they are laid upon the paper, &c. It renders blue, purple, red, green, and all other delicate colours, more bright and durable; and, if a small portion of it be added to any of the colours, it causes them to wash more freely and evenly over the surface of the paper, ivory, &c.

Combined with gum arabic, it gives depth of tint, without any unpleasant glossiness upon the surface of the drawing, and prevents the gum from cracking; and the colours are so completely fixed in the paper itself, that subsequent tints can be washed over them without any risk of their becoming foul, or forming improper combinations with the under colours.

Combined with fine lamp black, and gum water, it forms a complete substitute for Indian ink.

If it be floated over the surface of drawings made with chalks,

or black-lead pencil, it fixes them firmly; and they may then be washed over with any water colours, previously mixed with a portion of it, without in the least degree disturbing the chalks or black-lead.

For miniature painting, being washed over the surface of the ivory, it completely removes its greasiness; and being mixed with the colours, it causes them to float freely thereon, and tints may be laid over tints, the colours being struck into the ivory.

For transparencies, oiled paper, being first washed over with the refined ox gall, and permitted to dry, water colours, mixed with some of it, will lie freely, and perfectly smooth upon it, and be so fixed, as not to wash up by the repetition of different glazings of colours, over each other; thus producing depth of colour.

In short, the valuable properties this refined ox gall possesses, make it equally applicable to historical, landscape, botanical, and natural history painting, as well as to colouring prints in general; and, by its readily combining with all the vehicles used in the preparation of water colours, and having no colour in itself, it enables the artist to paint with ease on surfaces otherwise unfavourable, at the same time rendering the colours more bright and durable.

Many other valuable properties will, no doubt, on trial, be found in this preparation; and the result cannot fail to facilitate and improve the art of painting in water colours; and, as the discovery of it is entirely new, it will in all probability be found applicable to many other useful purposes.

May 20, 1812. P. W. TOMKINS.

LXXV. *Memoir upon the Causes of the long Duration of the Chinese Empire. Read to the Philotechnic Society, 2d of May* 1813. *By M.* PAGANEL, *Member of several learned Societies**.

In order to confine my subject within the bounds which circumstances require, I shall rapidly run over the traits which characterize all the nations of antiquity, and which distinguish them from those of Europe. Passing afterwards from these general considerations to the particular character of the Chinese, it will be acknowledged that this ancient nation is not less distinct from those who occupy the opposite extremity of Asia, than the latter are from the Hungarians and Germans.

Institutions in general derive their chief force from local conveniences; that is to say, from the concordance between those

* *Magazin Encyclopédique*, 1813, tome iv. p. 88.

institutions

institutions and the climate: a truth the more evident with respect to the Asiatic nations, whose moral character is derived among each of them from the physical qualities of the zone under which they are placed. This is the reason why the settlement of the Turks in Europe, and the translation of the seat of empire to Constantinople, are regarded as a violation of natural order, and as at variance with the principles and object of the Koran.

Certain traits of character and usages common to all the people of Asia seem to indicate that all have a common origin. In order to find any very striking difference between them, they must be compared in the two extremities of this great portion of the globe. Among all, their manners are as ancient as the societies themselves: as well as the governments, the latter have an aspect nearly similar, and present the faithful expression of the influence of climate. Despotism is an incontestable effect of this irresistible influence; and Mahomet, confounding the precepts of religion with the rights of the throne, confiding the duration of the empire to the human passions, formed out of the confidants of his impostures the nucleus of an invincible army, and of all the believers he made blind idolaters. Mahometism has lost nothing in Asia of its force and intolerance.

But Asiatic despotism is variously modified, from the east to the south. In vain would you endeavour to render it sanguinary in China, and paternal where the religion of Mahomet prevails*. At the two extremities it would overcome all obstacles, and return to its primitive state. Conquest or other circumstances may compress the spring, but they can never break it. It derives its strength from the agreement of human institutions with the moral character of men, their natural or factitious passions, and with the means of satisfying them.

In support of this opinion, I shall cite some celebrated facts. If the first caliphs, tempering the spirit of conquest by the noble

* I do not pretend to say that despotism is stripped of its essential character in China. There, as in the other Asiatic states, it is divided into graduated tyrannies, each of which oppresses and is oppressed in its turn. But in China, more than any where else, the sovereign watches over the immediate depositaries of his authority or his confidence: there, more than any where else, he occupies himself with his people, watches over their subsistence, and protects them against the extortion of the governors, who in China are more apt to elude the law and to defy all vigilance, on account of the system of despotism weighing entirely upon them. It is true, therefore, as I have said a little further on, that the despotism of the Chinese emperors is active, paternal, and popular, as much as absolute power can be. Some eminent writers have given credit to very contrary doctrines with respect to China. The enthusiastic admiration of some, and the contempt of others, for the Chinese, has equally impeded the investigation of truth. I have endeavoured to steer a middle course between these two extremes.

protection which they granted to learning and science, raised the Arabs of the East to the rank of the greatest nations; other Arabs developed, in the climate of Spain so favourable to genius, an imagination still more brilliant, more taste in the arts, and more ardour for the sciences. The glory of the Moors is attested to the world by more durable monuments, and by results more general. But these aberrations from the sloth and ignorance inculcated by Mahometism were but ephemeral. Sooner or later it recovers its fatal empire over the human mind.

It is possible that Asia may have been inhabited before every part of the globe. But I am very far from considering it as the only cradle of the human race. This opinion would imply that certain great questions had been resolved, which not only are not, but which probably never will be. For, if the human race is composed of races essentially different (an opinion more philosophical and more generally adopted of late years), must not each of these races have had its first man*?

What can never be contested is, that nature has heaped favours upon Asia: its climate excludes numerous evils which every where else beset the paths of men.

Shall I venture to say that happiness is there more constant and more easy of attainment, because human knowledge is there confined to a limited sphere, because Providence has refused to Asia in general the passions which are allied to the splendid gift of talents, and even genius itself? This is not the place to inquire wherefore this sublime faculty of the human understanding is indigenous in the temperate zones. It is sufficient for our purpose to know that nature does most for man where he is able to do least for himself. Admirable solicitude, which extends over every organised being! Harmonious benevolence, which more than the harmony of worlds upon worlds reveals to our feeble reason a supreme Intelligence!

Let us acknowledge, I say, that in Asia the enjoyments of life are placed nearer human wants, and as it were within the reach of every man's hand; that, besides, man is condemned to create and to conquer every thing; that under different skies he is the slave of the climate, or he becomes its master; that Europe is a theatre with constantly varying scenes; that the human mind as well as empires there undergoes astonishing revolutions; that it descends to extreme degradation as in the tenth century, and ascends to the summit of science as in the 18th; that every thing is in motion; and that it experiences without interruption the alternations of strength and weakness, enthusiasm and stupor, alternatives which depend on the use which is made of the powers of the human mind. That in Asia, on the

* None but pseudo-philosophers will ever adopt this method of accounting for the apparent varieties of the human race.—EDIT.

contrary,

contrary, the human mind exhibits no different phases; that the moral order is there uniform and constant, maxims and usages invariable; and finally the laws, religion, and government there triumph over the irruptions of barbarians, war, proselytism, and even time itself.

Let us now apply these general principles to the empire of China, which more than any other Asiatic state exhibits the happy results of local assimilations, that is to say, of the accordance of institutions and climate, and we shall perhaps succeed in resolving the historical problem now under consideration.

We may, without going too far, suppose that the civil and religious institutions of China were very ancient, even when they began to write the history of that country; and the history of China, according to M. de Guignes, goes back to the year 2953 before our vulgar æra. Confucius, who lived several centuries before the reign of Augustus, developed and inculcated the precepts of pure morality: but he certainly was not the first who instructed princes and subjects in their duties: the Chinese annals go back more than 5000 years; and probably at that epoch the science of morals and agriculture had attained in this vast empire a kind of perfection.

It is here important to remark the anteriority of the Chinese over the rest of the world in governing and in feeding mankind, as well as the limits of their knowledge in the arts of imagination, and in the sciences which bespeak all the vigour of intellect and all the fire of genius.

In order to be five thousand years ago what they are at present, the Chinese had only to collect the lessons of experience, always more forward in a paternal government, and under a sky whose serenity and mildness inspired the desire of pleasure; experience more certain under a paternal government protected by patriarchal manners, and which a fertile soil exempts from the dangers which an inconstant temperature occasions in other regions*.

Every thing which human ingenuity can invent and execute for procuring easy enjoyments to limited desires has been effected by the Chinese; but they will never quit the track which has been marked out for them by nature. Their affections retain them in some measure near instinct: they trace its progress, and it directs in turn their industry: this is not the case with the bee and the beaver. But the former stop, like these animal architects, at limits which they will never exceed. Ought we to complain that the inventive arts have exhausted their patience when they have scarcely seized upon the first

* The government has not yet succeeded, and perhaps never will succeed, in establishing a just balance between the subsistences and the immense population of the empire.

rules,

rules, and the infinite domain of genius is for ever closed against them? Ah! by how many advantages is this magnificent privilege compensated among these nations who are deprived of it! The Chinese are born for a life of indifference and tranquillity. If perfection in the arts, extent and depth in the sciences, are withheld from them, they are also ignorant of the revolutions and catastrophes of Europe. No ambitious views interrupt the stream of their enjoyments. They belong to the soil, like the plant which feeds them: they never experience the torments of curiosity; and with respect to them, their *caste* is that of the human race, and China the universe.

In China in general the epochs of life are feebly marked by the advancement and decrease of intellect: and if we descend to the lower classes, we shudder at beholding immense generations confusedly pressing upon each other, and continually disappearing in a fixed state of inveterate infancy. For this reason the vices of the multitude in China exhibit fickleness and imbecility, and for this reason also they abstain from the perpetration of those crimes which sully the annals of so many other nations.

Let us compare this condition of the Chinese with that of the people who were formidable by the glory of their arms, and celebrated by the more solid conquests of their genius; and we shall find on one hand a long series of happiness without noise, and on the other, some brilliant epochs purchased by centuries of calamity and crime; we shall find also (a problem which it would be difficult to solve) that man is happy wherever the laws reward the useful arts, and that warlike or highly cultivated nations are so much the more removed from happiness as they are greedy of glory or renown. We can easily reckon the happy days of ancient Rome, but we cannot count the days of sorrow and ignominy by which she expiated the conquest of the world.

The civilization of nations is slow, gradual, and frequently interrupted by a diversity of circumstances. We may therefore reasonably suppose that the civilization of the Chinese goes back to an æra far anterior to the reign of Fohi. In his time they were subject to the same usages, the same laws, and the same form of government. Here we may observe that several things seem to have been established among this people to separate them from all other nations: their language is a particular instance. To reproach the Chinese with the imperfection of their language, is to be ignorant of the spirit and object of their institutions. An uniform system of education, a complicated etiquette, superstitious ceremonies, a life of effeminate indolence for the fair sex, painful for the poor, sumptuous for the rich,

rich, voluptuous for both sexes; every thing in China is subject to a law of nature, because nature herself is there uniform, constant and absolute in her wishes, and leaves nothing to man but the wish to preserve his life and to enjoy it. But what has preserved China, and will always, from every innovation contrary to the spirit of the government, its usages and laws, is the place which it occupies at the further extremity of the globe.

Thus for a long series of ages has moral and physical order been reciprocally reflected and protected in this vast empire. Thus has the social accordance of human institutions from generation to generation, made the necessity of a paternal government more apparent; and all the parts of the empire are so happily combined, that they present to the eye and to the mind nothing but unity of movement, thought and desires.

The Chinese fully enjoyed all the benefits of their government until the time when the Tartars made themselves known to them by their sudden inroads. They then experienced the scourge of war, and all the evils which it brings in its train; but they were still for a long time ignorant of the military art. Enervated by a long enjoyment of peace, past events left but few traces on the memory, and they occupied themselves but little with the future. As invasions however became more frequent, they quitted for a moment this state of listlessness; but they proved by their defensive means, how easy it was to subjugate them, and when vanquished, how impossible it was for their conquerors to avoid submitting themselves to the laws of the empire.

The climate therefore has prescribed to China its government, usages and manners. But institutions dictated by nature ought to be consolidated from the instant of their birth, and to be perpetuated without contradiction. Rarely have wicked princes disgraced the throne of China. The Chinese, like other nations, may groan under the abuses of power, but they never hate power itself. The strength of the state consists in the invariable accordance between the wants of man and the action of the government—it is to be found in the harmony which reigns among all the parts of this vast edifice. And what ought not to be the authority of a system of legislation, which without tyranny is nevertheless despotic, and has regulated for five thousand years the destinies of an immense population *?

If the lust of conquest had not united under one chief the Tartars adjoining this vast and pacific empire, no revolution

* Fifty millions according to Voltaire, but one hundred and fifty millions according to the calculations of M. de Guignes, who in this point is entitled to most credit.

would have to this day chequered its history, and a conqueror would never have been seated on the throne.

The Manchou Tartars several times ravaged the provinces, before carrying their inroads into the heart of the empire. The government did not oppose numerous armies to the enemy, but constructed the Great Wall: a feeble rampart against valour and cupidity; and a memorable monument, according to Voltaire, of the love of peace. The Chinese never dreamt that the facility with which they might be conquered inflamed the desires of their invaders, and that the hands employed in raising this useless barrier would have been sufficient to immolate every Tartar at the frontiers of the empire: so true is it that the habitudes induced by the gentler passions have expelled from the breasts of this people the turbulent workings of hatred and revenge.

One thing is certain, that the emperors of China have raised militias or formed regular troops for the purposes of defence only. History does not reproach them with a single aggression*. With the interests of states, of which the European cabinets have formed a distinct science, they were unacquainted. Their policy was confined to the maintenance of internal tranquillity, the banishment of every new opinion, and of every doctrine contrary to the laws of the empire. They were insulated from the rest of the world, convinced by dire experience, that under the veil of commercial speculations, Europeans concealed dangerous projects. Nevertheless, by constancy and importunity, the latter secures the privilege of trading with certain points of the empire, but under restrictions and conditions so humiliating, that the love of money only can induce men to submit to them.

The Chinese rather tolerate than desire the presence of strangers: every where authority is exerted to keep them in awe, and no where are they received with hospitality.

From this primary cause arising from climate of the long duration of the Chinese government, other secondary causes flow, whose action not less constant fortifies that which is exercised by the climate.

The paternal regime is indigenous in China, as well as absolute power: this regime is sacred with respect to the prince, and this power is revered by his subjects as a law of nature. It is there that the justice of the head of the state, incessantly en-

* The emperors had frequently, and particularly those of the primitive dynasties, troubles to appease and rebel vassals to keep in submission. A kind of feudal hierarchy was the principle of these intestine wars. The supreme power was successively concentrated, and it is now and will be long an object of adoration with the Chinese people, as the source and the guarantee of the easy happiness for which nature destines them.

lightened

lightened by a long hierarchy of authorities, circulates equally among all ranks*. It is there that the citizens of all classes, chained to the chief of the state by an immutable order of things, obey him implicitly as children do their father. Thousands of centuries have elapsed, and the national character has experienced no sensible alteration. Its constancy is that of the climate. This beautiful feature, which embraces all institutions and all interests, commands so much the more admiration and respect, that the most ferocious conqueror has always bowed his head, and lowered his spear, when he contemplated it.

The Chinese government recompenses useful labours and virtuous actions by the highest dignities. This noble and rare justice is the principle of reciprocal confidence between the prince and his subjects, and consequently one of the causes of the duration of the empire and the government.

Some philosophers have deplored the lot of the Chinese, governed by a despotic power; but these critics do not see that the real despot is the climate. They do not see that this despotism is justified by a constant and paternal beneficence; that nature prescribes to it at the same time duties and limits; and that the history of the empire, written from day to day under the eyes of the prince by incorruptible magistrates, incessantly presents the faithful picture of his life and the judgement of posterity.

The Chinese are also reproached with the vilest cruelty, and the barbarous custom of arresting by infanticide the too rapid progress of population.

In condemning such usages let me be permitted to observe, that the severity of punishments in China is a necessary consequence of despotism, as despotism is in its turn a consequence of the influence of the climate; and that is moderated perhaps too much by the power of getting rid of it, by paying contributions graduated according to the magnitude of the crime and the wealth of the criminal.

As to the exposure of new-born infants, it has been tolerated only under circumstances which compromised the safety of the state: it has never been expressly permitted: it would be even punished, if the fathers, whose extreme poverty dictates such barbarous sacrifices, did not elude the vigilance of the magistrates.

Let us not judge the Chinese according to our learned opinions and refined abstractions: the former keep within bounds, they did not fall into error from going too far; but we, audacious reasoners, sometimes leave truth behind to run after brilliant theories.

* Such at least is the spirit of the institutions and regime of China.

In the privileged regions where the human mind is developed in force, in penetration, in extent, and where the imagination is inflamed by that creative fire which is called genius, man, by a fatal compensation, wanders in the wildest paths of error, and precipitates himself into the most humiliating excesses. It is there that in one and the same head are united a strong wit and a timid soul, boldness of thought and humble credulity: it is there that man is to himself an inexplicable phænomenon of oppositions and contrasts; it is there that Bossuet armed himself with the most profound dialectics to combat mere chimeras, that Pascal fixed the language extended the domains of science, and dug for himself a hell;—and it is there that the genius of Newton fell from the throne of the universe, repenting that he was seated there. It is in short upon this theatre of Europe, where Nature has so often crowned the sage who has surprised her at work, and the artist who knows how to imitate her,—upon this theatre covered with the trophies of genius, that genius too often creates false doctrines for ardent heads, and wanders far from the object of civil and religious sociability.

This balance of good and evil, which genius produces, is unknown, and will always be, in Southern Asia. The superstitions of the first ages are there at present civilized, and, if I may be allowed the expression, identified with the manners and usages. China is peopled with fantastic beings, with good and evil genii, because there must be in all places where men exist, fables and bonzes; and the bonzes must have riches and power*.

The Chinese have also their golden age, their deluge, and their Bacchus; they have revelations, mysteries, and prophets; but these errors are only the playthings of an infant people: superintended by the government, respected by learning itself, in China they kindle no civil wars, nor priestly quarrels. The word *intolerance* is no more in their language than is the idea expressed

* The constant serenity of their atmosphere invited the first people of India, those of Chaldea and Egypt, to observe the stars and their relation to the earth. But these very people, whom frequent catastrophes in the infancy of the world inspired with terror, soon interrogated the heavens instead of observing them. The chiefs of the nations could at their pleasure make the gods speak, and the priests arrogated to themselves an exclusive and mysterious science. But latterly astronomy has banished from its noble domains the vain formulæ of astrology. Among all civilized nations the celestial phænomena are explained by the laws of nature. Among the Chinese alone, the art of reading the destiny of men in the heavens, retains its empire. Their intelligence refuses to keep pace with the progressive improvement of the human mind. The government has collected some fruits from the missionaries, but these fruits have not spread among the great mass of the people. The astronomical observations of the Chinese, however, go back to the year 1122 before our æra.

by

by it in their minds. When in the heart of their peaceful empire the Jesuits and the Dominicans accused each other of heresy and impiety, the Chinese did not see two parties animated with a false zeal for the new religion; but these monks appeared to them as dangerous fools, or political agitators from whom the state must be freed. Such men, in fact, would have long braved the influence of the climate and every other influence. The Jesuits wrought with a profound and crafty policy, flattered the confident credulity of the prince, and humanely twisted the divine word to accommodate it to the prejudices of a people who were slaves to their habitudes. The Dominicans, energetic preachers, as if they still pursued from town to town, and from province to province, the heretics, armed themselves with all the arguments of the schools, proclaimed a holy war, and resolved upon the overthrow of China, rather than suffer the smallest departure from the doctrine which they were sent to propagate *.

The cause of the line of conduct pursued by the Chinese government on this occasion was the natural moderation of the minds of the people. Neither the interests of earth nor of heaven could inflame them to enthusiasm, and far less to religious fanaticism; a blind passion which absorbs every other sentiment, which courts darkness, which combats peace as well as light, and becomes more insatiate the more its appetites are fed. The dreadful reign of fanaticism has covered with ruins the states which are embellished by the arts and sciences, which thus have had to fight their way against intolerance and envy.

Fanaticism has made Europe for several centuries, and even since the revival of letters, the theatre of the most sanguinary wars, and of the most scandalous impieties. Extending its ravages with its doctrines, it has deluged a new hemisphere in the blood of its inhabitants. In China, on the contrary, the unity of thoughts and desires, the consequence of a limited intelligence, perpetuates, without any obstacle, union, peace, and happiness. And why? Because there reigns an admirable intelligence between nature and the social institutions; because good sense is the wit of the Chinese, and common utility the object of their industry; because the serenity of the air stifles every germ of curiosity, ambition, and heroism; because, in short, every thing there is stationary, both men and things, so that the government of China appears seated on the throne of Time.

* It results from my opinion as to the influence of climate and the intelligence of the Chinese, that they would have derived but feeble advantages from the settlement of our learned missionaries in their empire. But it is presumable that the people of Europe would have derived great benefits both with respect to the arts and commerce as well as with respect to the natural sciences.

If the fables which charm the vulgar in China could have filled their brains with fanaticism; if the reason of the Chinese had been so far deranged as to pretend that the worship of the bonzes is the only one which is agreeable to the divinity, and that the Father of mankind has chosen the emperor of China to convert all the world, or to cut their throats; Asia would have been burnt many centuries before fanaticism carried fire and sword to the innocent cities of the Incas. Ere the Europeans were in political existence, the Chinese had discovered the composition of gunpowder, which we have converted into a weapon of death; whereas in China it is only used to give éclat and solemnity to public rejoicings.

It is equally well ascertained that the Chinese invented printing many centuries before Germany attributed the honour of this discovery to herself, and that they confined the use of it to transmit from age to age the lives of the sovereigns, and the annals of the empire.

Printing, on the contrary, was scarcely known in Europe, than it opened all the schools to the most frivolous disputes. Down to the 17th century it was less serviceable to letters than to the propagation of lies and errors.

Thus all the secondary causes of the duration of the government of China bring us back to the primary cause of this singular phænomenon, the influence of climate. This influence, which equally affects the moral and physical powers, marks limits to the latter, which the Chinese neither have the power nor the wish to exceed. Their minds are constantly directed to what is useful, and there remain fixed: experience is their guide, good sense their companion, and they never desire a better condition. They can conceive none preferable to their mild state of servitude. Of all our opinions, that which would astonish them most, even their learned men, is our opinion as to the dignity of man, his honour, and liberty. With sound ways of thinking, the Chinese therefore never experience violent passions; their tastes, wants, and institutions bear the impression of uniformity, and of a succession of ages, which the authentic history of the empire has not been able fully to describe.

India presents a far different spectacle to the traveller, who cannot take one step in this ancient and primitive country without recognising the fatal effects of the revolutions which it has undergone since the people of Europe began to dispute for it as their prey. A single people now holds it in the most complete subjection. A merchant born on the banks of the Thames imposes tributes on the Indian princes; and their thrones have been levelled perhaps by a tradesman, who does not know how to manage any thing but a counting-house.

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In this rich portion of the globe, the inhabitants of which were civilized and learned when Europe could scarcely boast of a few market towns, traditions are lost, recollections effaced, and we can only grope our way through heaps of ruins. And whence comes this difference in the destiny of India and China? The one country has been able to insulate itself and to maintain its independence; while the other, on all sides accessible to navigators and to Asiatic conquerors, has only changed masters since the days of Alexander, and striven without effect against the tyranny and cupidity of its conquerors.

It results from all that I have said, that the climate of China has given birth to its institutions, and its geographical situation to its independence; that the influence of the climate is the primary cause; its isolation the preserving principle of its manners, laws, usages; and that we ought to refer to their combined effects the duration of the empire, and the longevity of its institutions, of which it offers the only example.

LXXVI. *Influence of atmospheric Moisture on an electric Column composed of Discs of Zinc and Silver. By Mr.* Thomas Howldy, *of Hereford.*

Hereford, May 17, 1814.

Sirs,—Although the experiments described in my former communication* fully demonstrated the influence of atmospheric moisture on the action of an electric column; yet, as the actual passage of the electric fluid over its exterior surface from the zinc extremity of the column to its opposite, was rather inferred from those experiments than directly proved by them, I must request you to insert (if this does not arrive too late) in the next number of your Journal the following experiments, which decisively establish that fact.

Experiment 1.—The positive extremity of the column was insulated, and the negative was made to communicate with the ground. The cap of a gold-leaf electrometer being brought in contact with the surface of the glass tube near to the positive extremity of the column, was left in that position for about two minutes; the leaves of the electrometer attained in that time a divergence of $\frac{1}{4}$ of an inch; the electrometer being withdrawn, the leaves still kept their divergence; their electricity was positive.

Exp. 2.—The cap of the electrometer was placed in contact with the glass tube at an equal distance from each extremity of the column. The leaves gradually opened to the same extent as in Experiment 1. with the same kind of electricity; the elec-

* Page 241 of this volume.

trometer

trometer being withdrawn, the leaves still retained their divergence.

Exp. 3.—The electrometer was placed in contact with the glass tube near to the negative extremity of the column. The results were precisely the same as in the two preceding experiments, except that in this the divergence of the leaves was *less* than in those.

Exp. 4.—The electrometer was now put in contact with the waxed end of the glass tube next to the brass mounting at the positive extremity of the column. In a few seconds the leaves just separated with positive electricity, but the electrometer being withdrawn, they closed. As the leaves were thus found to be only *influentially affected*, the electrometer was replaced in its position; and after the space of a few minutes the leaves diverged something more than $\frac{1}{8}$ of an inch: the electrometer being withdrawn, the leaves remained positively electrified.

Exp. 5.—The electricity of the waxed end of the tube next to the brass mounting at the negative extremity of the column was next ascertained. The leaves at first were only influentially affected; but after the contact had been continued for several minutes, they remained positively electrified.

Exp. 6.—The negative extremity of the column was now insulated as well as the positive. The same places of the column were examined as before, and the results were similar to the preceding; the only difference being that the divergence of the leaves was much less than in the former instances.

Exp. 7.—The column was now taken and exposed to the fire for about three minutes, in order to dissipate all the mois ure from its surface; it was then placed as in Experiment 1. and the electrometer applied to the different points in the manner already described. The kind of electricity found at each point was as before positive; and now in no instance was any electricity communicated to the leaves, which diverged only while the electrometer was in contact with the column; their divergence was very small.

I am, gentlemen,

Your obedient and obliged servant,

To Messrs. Nicholson and Tilloch. THOMAS HOWLDY.

LXXVII. *On Electricity: in Answer to Mr.* SINGER'S *Remarks. By* EZ. WALKER, *Esq.*

Lynn, May 18, 1814.

SIRS,—MR. SINGER still maintains that I have fallen into *error*, in my experiments on inducted electricity, and has advanced three statements to prove it*.

* Phil. Mag. vol. xliii. p. 20.

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The first consists of a reprint of a paragraph from one of his former papers*, which contains nothing but Mr. Singer's positive assertions. Mr. S. then adds, "these are *facts*, which the constant repetition of such experiments professionally enables me to state with confidence."

But philosophical statements must be demonstrated, either mathematically, physically, or experimentally, ere they can be received as *facts*. For, as the bare word of Newton would never be taken on a philosophical subject without a demonstration, consequently the assertions of Mr. S. cannot be admitted on any other terms.

Secondly, Mr. Singer says, "the statement in the preceding paragraph may be verified in a few minutes by any one sufficiently acquainted with the practice of electricity to make the experiments with due accuracy; and *I believe* it not only adduces a *fact* in proof of Mr. Walker's *error*, but offers the requisite information to show him its cause."

But the word *belief* is very objectionable, although it is the foundation on which Mr. S. builds his statement. The geometricians have not suffered this word to stain their pages once in 2000 years: and the experimental philosophers have, long ago, blotted it out of all their demonstrations.

Had Mr. S. understood the true meaning of this word, he would not have used it in the sense he has done. He has advanced it as a sound argument against my experiments; but will he admit it as an argument against himself? Let us try the experiment. If any one should say he *believed* that Mr. S. is a very superficial reasoner, it is ten to one that Mr. S. would admit the word belief, in this case, as a demonstration of the weakness of mental faculties. And if he will not admit this word as an argument against himself, he ought not to have used it against my experiments.

And thirdly, Mr. S. observes, "independent of this circumstance, it is *amusing* to find an *individual* so confident of the infallibility of his own observations, as to consider them sufficient to subvert the experience of Canton, Franklin, Wilke, Æpinus, Cavallo, Stanhope, and Robison.

Whether Mr. S. intended this last statement as a specimen of his wit, or of his reasoning, I know not. But as Mr. S. will not, I presume, be offended at my mentioning the name of a great astronomer on the same page with his own, let it, therefore, be supposed that Mr. Singer had lived in the days of Copernicus—Mr. Singer might then have said, that "it was very amusing to find an individual so confident of the infallibility of his own observations, as to consider them sufficient" to prove

* Phil. Mag. vol. xlii. p. 264.

that

that the earth moved round the sun,—when *all the world* believed that it was stationary.

I am, gentlemen,

Your obedient servant,

EZ. WALKER.

To Messrs. Nicholson and Tilloch.

LXXVIII. *Process for preserving the Canvass in Oil Paintings, and repairing Defects therein.* By Mr. CHARLES WILSON, *Worcester-Street, Borough*.*

1st. SEPARATE the canvass from the pannel, or straining frame, and lay it on a smooth table, with the painting downwards, and nail it securely.

2d. Take a piece of tin foil, larger than the canvass, place it on a very smooth table, and make the tin foil as smooth as possible with your hand. Then melt some Salisbury glue, in the same manner as for cabinet-makers' use.

3d. Warm the tinfoil before the fire, and lay it again on the table, then wash it over with the glue, and place it on the back of the canvass, secured as above, as quick as possible; smooth it perfectly with the hand, and let it remain in a warm room to dry.

4th. To repair the cracks of the canvass, in an old oil painting, lay it on a very smooth table, the subject downwards; then, with a brush or fine linen, cover the canvass with some melted white wax, and, with a warm flat smoothing iron, rub over the wax, and press it hard, which will draw the colours up to the canvass.

5th. To varnish the painting, clean the picture well, take some white wax, and spirits of turpentine, with a small quantity of linseed oil and sugar of lead; melt them over the fire, dip a fine linen rag therein, with which wash your painting; then, with a fine linen rag, rub over the varnish till it begins to be polished; let it remain till next day, and then rub it over with a fine waxed cloth, and afterwards with a soft linen cloth, using them alternately, by which means the painting will receive a very fine polish.

By the above means, the cracks and small holes in old paintings may be closed and repaired, and a coat of tin foil may be afterwards glued on the back of the canvass, as above mentioned.

A foot square of the tin foil costs about sixpence; when wanted of a larger size it will cost considerably more in proportion. It may be procured in sheets of three or four feet if wanted.

* From *Transactions of the Society for the Encouragement of Arts, &c.* for 1813.—The Society voted ten guineas to Mr. Wilson for this communication.

LXXIX. *Experiment on Respiration which had nearly proved fatal.—Beneficial Effects of Oxygen Gas in restoring suspended Animation.* By SAMUEL WITTER, *Esq. Dublin.*

Dublin, May 17, 1814.

SIRS,—THE following case, which occurred very lately in the laboratory of the Dublin Society, having excited no small degree of interest in this city, I am induced to transmit you a brief detail, in hopes it may prove both interesting and useful to many of your readers, convinced that every communication tending towards the enlargement of our knowledge in this regard may ultimately produce the most beneficial effects.

When a mixture of carbonate of lime and zinc, or iron filings, is exposed to an intense heat, the peculiar gaseous substance named carbonic oxide is disengaged, which has been stated to bear the same relation to carbonic acid that nitrous gas does to nitric acid. But agreeably to the striking observations of Mr. Higgins, professor of chemistry to the Dublin Society, in his work recently published, wherein his claim to the discovery of the atomic system is unequivocally established, it would appear that, in the combination of oxygen with different bases, it is the atom of oxygen only that is found multiplied, as is beautifully exemplified in all the metallic oxides, acids, and gases. An apparent anomaly has been noticed with respect to nitrous oxide, which the experiments of Mr. Higgins on the composition of nitrous gas tend to obviate, and sanction a comparison of the proportions of carbon and oxygen in carbonic oxide with those of azote and oxygen in nitrous oxide, rather than the atomic coincidence of carbonic oxide and nitrous gas. Carbonic oxide was discovered and described by Mr. Cruickshank in 1801; it is highly combustible, burning with a fine blue flame, but is utterly incapable of supporting animal life!

The diversified experiments of Sir H. Davy on the respiration of nitrous oxide and some other gases, so interestingly described in his scientific researches in 1800, in a great measure dissipated the general apprehensions of fatality resulting from the inhalation of compound gases, and satisfactorily demonstrated that many of the aërial fluids, before considered as destructive to vitality, might be breathed with perfect safety.

Desirous of witnessing the progressive effects of carbonic oxide when freely respired, with a view to comparative analogy in reference to nitrous oxide, I was tempted a few days ago to inhale a portion of it as copiously as possible. The consequence had very nearly proved fatal to me. A considerable quantity of the gas having been carefully prepared by Mr. S. Wharmby, the very ingenious

ingenious and able assistant to Mr. Professor Higgins, a series of experiments on its respiration were proposed. Mr. Wharmby first noticed some points of resemblance it bore to the nitrous oxide, particularly the singularly sweetish taste, and, having made two or three inspirations, was seized with a degree of convulsive tremor and giddiness that nearly overpowered sensibility. These violent effects were but transient, though considerable languor, head-ache, and debility, remained for many hours' afterwards. Anxious to pursue the experiment still further, I next made three or four hearty inspirations of the gas, having first exhausted my lungs of common air as completely as possible. The effects were an inconceivably sudden deprivation of sense and volition. I fell supine and motionless on the floor, and continued in a state of total insensibility for nearly half an hour, apparently lifeless, pulsation being nearly extinct. Several medical gentlemen being present, various means were employed for my restoration, without success; when the introduction of oxygen gas by compression into the lungs was suggested, the effects of which may be fairly contrasted with those of the carbonic oxide. A very rapid return of animation ensued, though accompanied by convulsive agitations, excessive head-ache, and quick irregular pulsation, and, for some time after mental recovery, total blindness, extreme sickness and vertigo, with alternations of heat and shivering cold, were painfully experienced. These unfavourable spasms were succeeded by an unconquerable propensity to sleep, which, as might be expected, was broken and feverish. An emetic of tartarised antimony finally removed these alarming symptoms, and the only unpleasant effects felt on the ensuing day were those occasioned by the fall.

I very much regret that the confusion arising from the idea of my death, so disturbed the arrangement that no accurate determination could afterwards be made, either of the quantity of gas respired, or the change it underwent in the process; and the experiment is rather too hazardous for repetition. Nevertheless, the extraordinary efficacy of oxygen gas in cases of suspended animation produced by carbonic acid, choke damps, and other suffocating gases, is fairly deducible, and, I conceive, cannot be too forcibly recommended to the faculty, in such instances. I therefore sincerely hope that the results of this experiment may be of practical utility in those cases, which are so frequently occurring, and are often so awfully fatal; it being the decided opinion of the professional gentlemen present on this occasion, that the free use of the oxygen gas was solely instrumental in restoring me to life.

Mr. Higgins himself had nearly once fallen a victim to a similar experiment with sulphuretted hydrogen, the effects of which, after

after recovering from a death-like insensibility, were painful and oppressive for many days.

I am your obedient servant,

SAMUEL WITTER.

To Messrs. Nicholson and Tilloch.

LXXX. *Case of Retention of Urine successfully treated by puncturing the Bladder. By* JOHN TAUNTON, *Esq., Surgeon to the City and Finsbury Dispensaries, and to the City of London Truss Society for the Relief of the ruptured Poor, &c. &c.*

SIRS,—JOHN JONES, aged 67, a brass turner by trade, a stout muscular man, his general health extremely good, with the exception of an ulcerated leg for the last twenty years, and during the last eight or ten hydrocele on one side and hernia on the other. For the last twelve months also he has experienced some difficulty in voiding his urine, which came away in small quantities at a time, and with frequent calls. On July 7, 1813, after a hearty dinner he sat an hour or two in the open air, during which time he was attacked by violent pain in the abdomen, with purging. These symptoms continued all night, and his urine came away in drops without any effort. On the 8th he was admitted a patient of the Finsbury Dispensary.—The abdomen was hard, swelled, and painful; and there was a considerable degree of fever attended with thirst: the purging also continued. The leg, which had been much inflamed for the last three months, is now better. An anodyne fomentation and some powdered rhubarb were prescribed for him by the physician who visited him till the 13th; and during the interval aperient fomentations, &c. were resorted to with a view to relieve the complaint in the bowels and general disorder of the constitution. On the 13th Mr. Taunton was requested to see him, on account of the stillicidium urinæ which had existed from the beginning of the attack. He found him with quick weak pulse, brown tongue, violent pain in the region of the bladder, which was distended, forming a tumour reaching above the umbilicus: the urine was dribbling away involuntarily. The catheter was introduced, but could not be passed beyond the neck of the bladder. The smallest gum catheters were also tried without effect. Continuata foment.

14th. The bladder reaches still higher up: the catheter again attempted, but in vain; other symptoms the same. Puncturing above the pubis was determined on, and consented to by the patient. Upon going to perform the operation at three o'clock, it was found that a considerable quantity of urine had come away involuntarily and almost in a stream, and the patient

would not now consent to the operation. Nor was it urged, as the bladder was greatly reduced in size. The tongue was still brown, and the other bad symptoms continued. 15th. The patient was nearly in the same state: stillicidium constant. 16th. The tumour formed by the bladder is more prominent and circumscribed; reaches about two inches above the umbilicus: in other respects the same:—pain decreased. The operation was now performed, and between two and three quarts of urine were taken away; it was not grumous, nor materially altered from that of a healthy person. During the operation the pulse fell, but soon regained its strength. A long elastic catheter was left in the wound, and properly secured. A cordial mixture was prescribed. In the evening the bougie used as a stilette was withdrawn, and the urine evacuated: no pain on pressure on the abdomen, which was soft, and the tongue clean. 18th. The urine escapes by the side of the catheter, but is not effused into the cellular membrane; adhesive inflammation was visible round the wound. The urine flows involuntarily, but he feels easy. He took broth yesterday: the tongue was clean, the pulse was stronger and slower. 19th and 20th. A slight blush of inflammation immediately around the wound: no pain experienced on pressure. 21st. Has felt pain in the night, seemingly from a temporary obstruction to the flow of urine, which was soon evacuated, and the pain went off. 22d. The catheter escaped during the night; but the urine flows freely from the orifice, and he continues to gain strength. From this period to August 28th convalescent. The passage of a small bougie has been attempted two or three times without success. The patient complains of considerable pain in the urethra, which prevents his sleeping without opiates; and he takes a grain of opium every night. He walks out, and his spirits are better. September 10th. The quantity of urine discharged by the urethra has sensibly increased, until it all comes away by that channel. The opening had closed a few days before, but it broke out when straining at stool; he does not know whether any urine escaped or not, but nothing comes from it at present. 17th. Much in the same state; complains of soreness where the puncture was made, and a little matter oozes from it; a considerable quantity of urine came through on the 13th and 14th. From this time he gradually recovered, and was discharged cured the beginning of November, since which he has not had any return of the complaint.

To Messrs. Nicholson and Tilloch. JOHN TAUNTON.

LXXXI. *Some*

LXXXI. *Some Particulars of the Life of Count* Bougainville, *the French Circumnavigator. By M.* Delambre, *Secretary to the French Institute**.

Louis Antony de Bougainville was born at Paris on the 11th of November, 1729. He was the son of a notary at Paris, and descended from an ancient family in Picardy.

A celebrated navigator, a general officer, member of the Academy of Sciences, of the Institute, and of the Board of Longitude, were so many titles which he owed to his own merit, and which were the recompense of a long series of illustrious actions.

While at college he was distinguished by an ardent desire for knowledge. His professor one day was explaining the phases of the moon, and its various positions: in order to impress his ideas on the memory of his auditors, he quoted two Latin verses to them. Young Bougainville was bold enough to consider them as of an inferior kind; and being challenged to make better, he answered almost instantly by four verses more accurate, more instructive, and more poetical, than the distich which he had criticised.

On leaving college he was admitted an advocate in the Parliament, by desire of his father; but in order to indulge his own inclination he inrolled himself in the musqueteers. Chance made him a neighbour to Clairaut and d'Alembert, and he attached himself warmly to these two geometricians; he visited them often, profited by their conversation and writings, and at the age of 25 he produced the first part of his Integral Calculus, to serve as a continuation of *De l'Hôpital's Infinitesimals*. With that candour which was always one of the most striking traits in his character, he declared in his preface, that *nothing in the whole work was his own, but the arrangement which he had endeavoured to give it*. The committee of the Academy, however, attested that by explaining the methods of the various geometricians, he had made them his own by the clearness and intelligence with which he elucidated them. In addition to this flattering testimony he found also another recompense in the certainty of being useful to young geometricians, who were greatly in want of guides to enable them to penetrate into this hitherto obscure branch of the mathematical science.

In 1755 he was made a major, and visited London as secretary of the embassy, where he was elected a fellow of the Royal Society. Next year he followed General Montcalm to Canada, with the title of captain of dragoons. And as a proof that so many and various functions did not make him neglect the sciences,

* *Magazin Encyclopédique* 1813, p. 315.

before he embarked for America he put to press the second part of his *Integral Calculus*, having requested Bezout to read the proof sheets in his absence.

Immediately on his arrival in America he marched at the head of a detachment amidst ice and snow, and through almost impenetrable forests, to the extremity of Lake Sacrament, where he burnt an English flotilla under the guns of the fort which protected it.

In 1758 a detachment of 5000 French troops was pursued several days by an army of 24,000 English. Bougainville inspired his fellow soldiers with resolution to wait for the enemy: they hastily fortified their position in less than 24 hours, and compelled the English to fall back with a loss of 6000 men. Bougainville was wounded on this occasion in the head by a musket ball. The French governor despaired, however, of saving the colony, if he did not receive reinforcements from home. Bougainville was sent to France to solicit them, and he returned with the rank of colonel and the cross of St. Louis, granted before the usual time in consequence of his brilliant services. Montcalm placed him at the head of the grenadiers and volunteers, to cover the retreat of the army which was forced to fall back on Quebec. He performed this important service with his usual intrepidity and skill. The death of the general hastened the loss of the colony; and Bougainville returned to France. He then followed M. Choiseul de Stainville into Germany, where he again signalized himself, and his bravery was rewarded with the gift of two pieces of brass cannon. The peace deprived him of further opportunities of distinguishing himself as a soldier, but it did not lessen his activity. We have seen him as a geometrician, a warrior, and a negotiator. We shall now view him as the founder of a colony.

His various visits to America had made him acquainted with the merchants and ship-owners of St. Maloes. A vessel which left that port at the commencement of last century had anchored on the south-east shores of a group of islands visited by the English, who had called them at first Virginia and Hawkins's Islands, but now the Falkland Islands. The favourable situation of these islands gave rise to the idea of forming an establishment there. The French court took up the idea in 1763, and Bougainville offered to commence it at his own expense. In concert with two of his relatives he fitted out two ships at St. Maloes, and embarked some families, with whom he reached the islands called the Malouines on the 3d of April 1764. They were inhabited; but no violence and no injustice attended his occupation of them. An abundant fishery, birds which at first permitted themselves to be taken with the hand, secured the means of subsistence; but

but no wood for fuel or erecting houses was to be procured. Rose bushes and excellent rich grass were found in abundance. The foundations of a fort were laid, and the walls were raised of earth. Bougainville set the example, and all the colonists took part in their erection: in the centre of the fort an obelisk was raised, and the hemistich "*Tibi serviat ultima Thule*" was inscribed under a portrait of the French king: another inscription exhibited the line in Horace, "*Conamur tenues grandia.*" When these first labours were over, Bougainville returned to France, leaving the government of the infant colony to one of his relations. Next year he returned with a supply of provisions and new inhabitants. An excursion to the Straits of Magellan procured him wood for the purposes of building, and ten thousand young forest-and fruit-trees. An alliance was concluded with the Patagonians; most kinds of the grain cultivated in Europe were naturalized, and cultivated with success: the multiplication of the cattle was a matter of certainty, and the number of the inhabitants rapidly increased from 80 to 150. But these acquisitions did not satisfy the active mind of the founder. They had alarmed the Spaniards, however, and complaints had been made by them to the French government. Bougainville was finally ordered to deliver up his possession, and the court of Spain agreed to pay him for his works, and to refund his expenditure. As a further consolation, the court of France appointed him to make a voyage round the world. The command of the frigate *la Boudeuse* was given him, and the store-ship *Etoile* was ordered to join him. The naturalist Commerçon and the astronomer Veron were embarked with him, at his request, to examine the new methods of finding the longitude.

It was on the third of May 1765 that Bougainville surrendered to the Spaniards the colony, which had been scarcely two years in existence, and of which he foresaw the speedy destruction: he regretted in particular the loss of an observatory which he wished to build there, and which from its position of 51° south latitude must have been an useful addition to the great observatories of Europe. While preparing to quit the island he saw a comet for several days, which had ceased to be visible in Europe. It was the second comet of 1766; and Pingré, who has carefully collected all the observations made of it at the Isle of France, seems to have been ignorant that it was mentioned in Bougainville's Voyage round the World.

Since his projects were overthrown his island became of a secondary interest to him: all his thoughts were turned to the brilliant expedition which he was about to undertake; but the store-ship which was to join him with provisions not having arrived, he thought that some obstacle must have occurred to prevent

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vent her making the Malouines, and he proceeded to Monte Video in quest of her. He resolved to take a range which could not be less than 800 leagues, and which was in fact 1200; for he must necessarily return and pass almost within sight of the Malouines, in order to penetrate into the Pacific Ocean through the Straits of Magellan. Scarcely had he arrived at Buenos Ayres when he witnessed the seizure of the Jesuits of Paraguay. He speaks like a man free from prejudice, and impartially relates every thing which can be urged in favour of the order; nor does he withhold the reproaches which it merits.

Seven months after his departure he found himself not far from the Malouines, opposite Cape Virgins at the entrance of Magellan's Straits. Here, by solar and lunar distances observed, he again determined the longitude, and ascertained the situation of the ship.

The passage of the straits was dangerous: thick fogs and impetuous winds compelled them to bring-to, and to take soundings incessantly, and the current frequently forced them further back than they had advanced. The fires kindled by the Patagonians assisted our navigators in making the land. They were well received by the natives, and Bougainville always retained a grateful sense of their kindness. This interview impressed him with a better opinion of their dispositions than he had formerly entertained. Here his discoveries commenced. The names given to the islands, bays, and straits in these latitudes are so many monuments of the exertions of the French for the advancement of science: but an unpropitious sky rendered almost useless the labours of the astronomer Veron, in the island which was called *L'Observatoire*.

Storms accompanied our voyagers until they left the straits. This passage, which Bougainville estimates at 132 leagues, occupied 52 days of a laborious navigation, which however did not affect the health of the crew, for on entering the Pacific Ocean no person was on the sick list. The navigation then became easier and more interesting: discoveries were made daily. To some of these Bougainville gave names, and of this description are the *Quatre Facardins* and the *Boudoir*. He perceived the latter island two days before reaching Taïti, (Otaheite). The necessity for coming to an anchor was then felt, and it would have been difficult to have found a more hospitable shore.

The anchorage, however, was unsafe, for it cost them six anchors in nine days.

What contributed greatly to the interest of this visit was the resolution of a young Otaheitean to come to Europe with our voyagers. He was named Aotourou; but was better known as *Poutaveri*, being his pronunciation of the name of his friend Bougainville.

gainville. This young man amused them much during the voyage: it was remarked that he gave names in his own language to the most brilliant of the stars, and he had made several voyages to the neighbouring islands, the positions of which and the manners of the inhabitants he described as well as he could. Aotourou remained eleven months at Paris. The desire to see him was great, and his patron neglected no means of making his stay in France agreeable. Aotourou repaid these attentions with the warmest gratitude, and by leaving a collection of anecdotes, which would have been always read with much pleasure if M. de Bougainville had introduced them into the account of his voyage. Nothing was omitted to secure his return to his native island in a respectable manner. The most costly presents were made him, and a sum of 36,000 livres was given him by Bougainville out of his own pocket. He was received with great attention at the Isle of France; and Capt. Marion, who was to convey him to Otaheite, also took the most particular care of him. He died of the small-pox, however, during the voyage. The same fate had befallen two other islanders who left Otaheite with an English captain, eight months before Bougainville.

After leaving this island, the voyage for a long time presented nothing interesting. The dangers which they met with alone interrupted the monotony of their nautical observations; and the most dreadful of all evils, famine, stared them in the face. The rations were reduced, the route was changed, and they renounced all attempts at discovering a passage which was long suspected to exist. The glory of this discovery was reserved for Captain Cook, who fell in with it most fortunately when his vessel was on the point of being lost. A similar danger awaited Bougainville, if the want of provisions had not compelled him to abandon the project. They escaped at length; a cape received the name of Cape Deliverance, but the scurvy now began to commit ravages among the crew. Fortunately a passage was found through the Papon Isles, and they entered the sea of Molucca. Bourou presented a most delicious anchorage, where, notwithstanding strict orders to exclude every foreign vessel, the resident permitted them to rest after so many fatigues. Aotourou, transported with joy at the sight of so many objects, asked if Paris was as fine as Bòurou; but his admiration was soon checked at the sight of the numerous diseased inhabitants contained in Batavia, to which he gave the appellation of *Enoua mati*—Land which kills.

From Batavia the ships proceeded to the Isle of France, from thence to the Cape of Good Hope, and subsequently to St. Maloes, where they arrived on the 16th of March 1769, after a

voyage of two years and four months, and the loss of only seven men out of more than 200.

This expedition justly placed Bougainville in the rank of the greatest seamen of his day, and yet it was in some measure his apprenticeship only. The account which he gave of it was read with avidity, and afterwards translated by Mr. Foster; for in a second edition, which he published in 1772, he answers some remarks of his translator. His style is simple and natural: he there exhibits his character, his intrepidity, his contempt for danger, and his penchant for pleasantry; his goodness of heart, and the gaiety with which he contrived always to enforce subordination, and yet to provide for the enjoyments of his crew as much as for their health.

It has been truly observed, that the geographical charts and determinations, with the exception of the latitudes, are the weakest parts of the work. But it is fair also to remark that he made a voyage of discovery, and not one of mere *reconnoissance;* that dreadful weather rendered all his astronomical attempts useless; that the science of the longitude was in its infancy; that the tables of the moon were not yet brought to the point of perfection at which they now are; that navigators then had none of the assistance which is lavished upon them at present; that they were still unacquainted with calculations; and that Bougainville was the first Frenchman who took an astronomer with him to profit by his observations.

Upon his return, France was at peace. A wandering and agitated life had blunted his taste for the mathematics, and he gave himself up to enjoyments which the bustle of his early life had not permitted him to share. His celebrity and his elegant manner procured him admission into the higher circles; but his active mind was again employed in the service of his country when France declared for America. Under Admirals Lamothe Piquet, D'Estaing, and De Grasse, he successively commanded the French ships *Bien-aimé*, *Languedoc*, *Guerrier*, and *Auguste*. At the request of D'Estaing he was appointed *chef d'escadre*, and the same year he received the rank of field marshal. He commanded the van at the memorable battle of the Chesapeake in 1781, and beat off the English van, obtaining the honourable testimony of Count de Grasse to his having contributed more than any other person to the victory. On the disastrous 12th of April, when the commander in chief was reproached with being more occupied with the safety of his own ship than with the squadron, and the squadron with not supporting their commander in chief, Bougainville, who commanded the rear guard, did all that could be expected of him: by a bold manœuvre he saved

saved the Northumberland; and although the *Auguste* which he commanded was one of the most roughly handled of the whole fleet, he collected and conducted to Saint Eustatia the remains of the shattered squadron.

The peace which secured the independence of America restored M. de Bougainville to that leisure which is so necessary for the pursuit of the sciences. The Academy conferred upon him the title of honorary member. M. Lagrange, whose vote he asked, observed: "To you it was that I was indebted for being received into the Academy, since your works opened to me the career which I pursued."

About this period he conceived the project of tracing the icy regions of the north, and penetrating to the pole. A distinguished astronomer offered to accompany him, and the route was sketched. The French ministry however did not accede to his terms, and the Royal Society of London asked him for his plans. He transmitted them immediately, pointing out the route which he would take. Captain Phipps, afterwards Lord Mulgrave, preferred another, one also of Bougainville's suggesting, but he could proceed no further than 80°.

When a spirit of insubordination broke out in the French navy, and in the Brest fleet in particular, M. de Bougainville, by his reputation, his courage and his firmness, mixed with the most amiable qualities, seemed to be the only man capable of recalling the seamen to their duty. But his exertions were unavailing; the flames of jacobinism had spread too far, and he retired from the service in disgust. In 1791 his name was put upon the list of vice-admirals. This high distinction redoubled his attachment to a prince who was abandoned by all. From the massacres of 1792 he escaped as if by miracle, and took refuge on his estate in Normandy, where he found his two pieces of cannon the only recompense which he had received for 40 years service.

On the restoration of order he was appointed to the Board of Longitude; but whether he did not think matters sufficiently settled or the care which it was necessary to take of his fortune prohibited him from leaving his estate, he sent in his resignation, and was succeeded by Count Fleurieu, who afterwards resigned in favour of M. de Borda. When the Institute was formed, M. de Bougainville was nominated to a seat at the Board of Navigation and Geography. As President of the Class of Sciences, it was his duty to deliver to the emperor the reports of that department, and he acquitted himself with great dignity.

As a senator his pecuniary circumstances were made perfectly easy; but although old age was coming on he possessed all the fire and vivacity of youth. He was still desirous of partaking in some hazardous maritime enterprise; and when his friends mentioned

tioned his age, he replied that Nestor was not altogether useless in an army which boasted such heroes as Achilles, Ajax, and Diomede. Although his temperance and sobriety were great, and we had hoped to have him long among us, he died on the 31st of August 1811, after a sharp illness of ten days.

He had frequently spoken to me of his intention to intrust all his memoirs and journals to my care. This promise prevented me in some measure from presenting myself before him when he was in danger, and I have lost much by this reserve. Let us hope that some more adequate person will profit by the possession of his valuable materials.

He was an excellent father and a warm friend. Incessantly occupied with the interests of science, he seized every opportunity of being useful to it. Frank and loyal in his disposition, he rose in life without intrigue, and conducted himself so as to deserve the esteem of all parties. He left three sons equally distinguished with their father for zeal in the service of their country. His place at the Institute was filled up by M. de Rossell, the companion, continuator and editor of D'Entrecasteaux's voyages.

LXXXI. *On the Errors in the Nautical Almanac.*

May 24, 1814.

SIRS,—SOME time ago the public attention was called (through the medium of your Journal) to some errors which had appeared in one of the Nautical Almanacs; and it was hoped that the observations then made would have induced the editor of that truly valuable work to have taken the requisite precautions to prevent the recurrence of any similar complaint. It is indeed with much regret that I now write to you, with a view of pointing out some very striking errors which appear on opening the Nautical Almanac for 1816, and which ought to be corrected as soon as possible; because a discovery of this kind very naturally excites a suspicion that other parts of the work may be equally inaccurate; the truth or falsehood of which I have not time to investigate at present.

To return, however, to the subject above alluded to:—In the Introduction to the Nautical Almanac for 1816, you will find that Septuagesima Sunday, Shrove Sunday, Midlent Sunday, Palm Sunday, Easter Sunday, Low Sunday, Rogation Sunday, Whit Sunday, Trinity Sunday, and Advent Sunday, are all represented as happening on a *Friday:* thus, Easter day (which really falls on Sunday April 14) is said to fall on Friday, March 29; Ash Wednesday is said to fall on a Monday; and Holy Thursday on a Tuesday ! ! !

The *Chronological Cycles* likewise are all wrong: the Dominical

cal Letters should be G, F, instead of E, D; the Lunar Cycle should be 12 instead of 8; the Epact should be 1 instead of 17; the Solar Cycle should be 5 instead of 1; and the Roman Indiction should be 4 instead of 15.

How far the other parts of the work may be correct or not, I have not time at present to ascertain: but as even these errors ought to be corrected as soon as possible, and as I do not know of any more ready method than by means of your widely extended miscellany, I hope I need not apologize for troubling you with this letter.

I am, sir,

Your obedient servant,

CRITICUS.

To Messrs. Nicholson and Tilloch.

LXXXII. *Notices respecting New Books.*

An Account of Baths, and of a Madeira House at Bristol: with a Drawing and Description of a Pulmometer; and Cases showing its Utility in ascertaining the State of the Lungs in Diseases of the Chest. By EDWARD KENTISH, *M. D. Physician to the Bristol Dispensary and to St. Peter's Hospital.* London. Longman and Co.

THE author of the above treatise has had the merit of being the first in this country to commence an establishment, which has long been recommended by medical writers, for the relief of valetudinarians*. His Madeira House at Bristol presents most of the comforts which have been repeatedly suggested as attainable in our climate, without subjecting persons of delicate or infirm habits to the expense and risk of a distant voyage.

The first care of Dr. Kentish was to provide a suite of apartments for baths, which promise to be of great service under the superintendence of a judicious medical attendant. We shall give the description of this part of the establishment in his own words:

"The entrance into the baths is by folding doors, by a plain unornamented portico, which opens opposite to the east end of the cathedral,—a large open area, very commodious for the access to the baths either by carriages, chairs, or on foot. The situation is extremely convenient, easily accessible to the inhabitants of Bristol, and to the visitors of the Hotwells and Clifton, who may have occasion to use the baths.

* Dr. Adams, the celebrated author of the work on Morbid Poisons, first suggested the idea of an establishment of this kind in a letter written while at Madeira, and inserted in the Medical Journal. Dr. Pearson (to whom Dr. Kentish pays a well-merited compliment) subsequently exerted himself to introduce similar improvements in our domestic œconomy.

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"The large room, in which the baths are placed, is thirty feet long, by twenty feet wide; the cieling is fifteen feet high; and the whole is lighted by a dome light from above.

"From the entrance at the portico, a few gentle and easy steps conduct into the servants' lobby, through which you pass into the bath room; this is divided by partitions, twelve feet high, into four small chambers, the remaining area forming a sort of waiting room,—large, well lighted, and provided with seats and a table, where the bathers may amuse themselves with a book until the bath is prepared; or, after the use of the bath, remain in a middle temperature previously to exposing themselves to the air.

"In two corners of the room there are large reservoirs; they are placed close up to the cieling, and are inclosed by partitions; they contain each about four hundred gallons, the one of hot, and the other of cold water, which is conducted by pipes into each of the bath rooms, terminating in the baths, which are made of copper, and japanned in such a manner as to imitate the verd antique marble. From each of the baths there is a waste pipe, which carries off the water when done with in the bath; these unite into one common pipe, which conveys it through the water-closets of the house. Thus the bather sees the water drawn fresh from the reservoirs for his own use, and may see it run to waste if he chooses. By these means the impossibility of having a bath which has been used is complete. Adjoining one of the warm baths there is a shower bath containing twelve gallons, which is charged with hot or cold water at the wish of the patient. Into this bath also are conveyed the hot or cold douches.

"The warm water reservoir is provided with a false bottom, about two inches from the real one; between these two bottoms a current of strong steam is thrown by the means of a small boiler placed in a room below the bath room; the water above the false bottom absorbs the heat of the vapour, condenses it into water, which falls back into the boiler, where it again receives a proportion of heat, and carries it back to the false bottom. By this process the water becomes heated in the reservoir to about 150 degrees of Fahrenheit's thermometer; it probably might be carried higher. This is as high as I have had it, and is much more than an adequate heat for any purpose of bathing. The main steam pipe which passes from the boiler to the reservoir, goes through the vapour bath room, from whence the steam is drawn in any manner which may be required; it is made to pass in any direction, and may be impregnated with any substances that might be desired. The various vapour douches, jets, and other modes of locally applying this power to different parts of the body, are so arranged as to be under the guidance

guidance of the assistant. This combination of local with the general use of the vapour bath, is capable of producing effects, which nothing less than having been a witness to them myself could have induced me to believe.

"In case the warm reservoir should have been too freely used, and the temperature should not be found equal to the required degree, each warm bath is provided with the means of being heated separately. One has a double bottom, between which steam is conveyed, which imparts its heat to the water, and may be thus raised to any temperature. The other warm baths are provided with a steam pipe, which descends to the bottom of the bath, and steam is thrown into the water, on the same principle as Count Rumford fitted up an apparatus for Mr. Gott of Leeds.

"The manner of heating water according to this mode of the Count's, is attended with several inconveniences. The steam going into cold water, is suddenly robbed of its heat, becomes condensed, and forms a vacuum; according to the quantity condensed, a corresponding report is produced. If the pipe is of three or four inches diameter, the report would be equal to the report of a musket or pistol. Another material inconvenience is the great vibration caused by these reiterated concussions, which in a short time would destroy the integrity of any machinery. To overcome these objections, I have consulted many artists and engineers, and have been at much trouble and expense in a variety of experiments. The means I have found the most effectual to overcome the difficulties just enumerated, is to have a small copper pipe of three quarters of an inch diameter, conveyed round three sides of the bottom of the bath; this pipe is to be perforated by an infinite number of small holes, not larger than the perforations made in the top of a copper watering pot; the perforations should commence in the pipe five or six inches after it has reached the bottom of the bath, and continue to the entire end; by this means the small bubbles of steam, passing through these small apertures, are condensed with little noise and very trifling violence. This is a very good mode of heating water, and very applicable to the heating of baths. The double bottom, in many instances, is better, particularly where the material would be injured by an addition of water: for the steam, which is thrown in by the steam pipe, is condensed into water, and adds considerably to the quantity of fluid in the vessel thus heated. This is not the case with the false bottom, as the water does not communicate with the bath, but is condensed and collected, being pure distilled water; which is applicable to a variety of useful purposes, for saline solutions, required in making artificial mineral waters. From this account may be collected

lected the various combinations of power in applying heat or cold which are concentrated in this establishment. Had I stopped here, the arrangement would have been incomplete. Modern chemistry enables us not only to decompose and recompose water itself, which was regarded as a simple element, but also to analyse, and form, by artificial combinations, compounds similar in every respect to the natural medicated springs. As this island, at all times, is wanting in some of the most powerful mineral waters which are only found upon the continent, and at present even the rich of our island cannot benefit from their use, I trust that I am doing an acceptable service to the public, when I enable them to receive all the beneficial effects, without the trouble or expense of a sea voyage, and a tedious journey amongst strangers in a foreign land."

After a concise description of the most celebrated baths on the continent, Dr. Kentish proceeds to show the analogies between the physiology of plants and animals, and treats of the effects of climate on animal and vegetable life. This leads him to another part of his plans for the amelioration of the condition of the infirm, viz. the Madeira House, of which he gives the following description:

"I adhere to this term in my description, as it has been announced to the public in the Prospectus republished in the first part of this Essay. The term Conservatory might be applied to it with more propriety. The intention is to have the power, in a building or house, of regulating the temperature in each apartment. As it might be wished to have a temperature much above that of Madeira, the term is therefore not strictly appropriate. The use of fires to warm houses, as well as to answer the purposes of cooking, bears date from the earliest times. Wood was for many ages the chief fuel. When coal was first introduced, it had many prejudices to combat before it was admitted into general use. The manner in which our houses were warmed for several centuries was perfectly devoid of all sound principles of philosophy: it was not until the latter part of the last century that philosophy deigned to apply its principles to the useful purposes of life. Society owes more to Count Rumford, for his investigations respecting the application of fuel to all useful purposes, than to any other. Not only are we indebted to him for what he has done, but infinitely more so for what he has caused others to do, by having given a direction to genius to employ itself upon the practically useful. Several methods have been devised for warming buildings:—the Russian and German stoves; flues, after the manner of hot-houses; iron pipes heated by steam; stone bottles filled with hot water; keeping cows in the room, &c. &c. These methods may answer

swer very well to produce heat, which may be marked by the thermometer; but more than this is wanted in a conservatory for invalids or delicate subjects.

"A supply of warm air must be had; but not merely warm air; it must possess all its vital principles. There must be a circulation of air through the apartments, that is, the air which has served the purposes of respiration and combustion must have a free egress, and fresh-charged atmospheric air be freely admitted; for it is proved by the experiments of Messrs. Lavoisier, Guyton, Fourcroy, and Davy, that the common atmospheric air, from the Frozen Ocean to the equator, consists of the same component parts; the only difference between the air of England and of the most favoured climate depends upon its proportion of caloric. The best means I know of answering this purpose, is by placing a stove in the lowest part of the house, which stove is to heat porcelain tubes, through which the atmospheric air will be conveyed into a large main pipe going from the bottom to the top of the building; from this pipe currents of warm air may be directed into any apartment, in the same manner as water is conveyed from a reservoir placed at the top of the house, in pipes, to all the various parts below its level, following the laws of specific gravity. As heated air has specific levity with regard to the atmospheric air, if we have a reservoir of heated air in the lowest part of the house, we may convey it from the bottom to the top, by the same laws as we do the water from above. This is the mode of heating the Madeira House, by thus throwing into any given apartment a current of warm air. If there be a small fire in the room, the air which that consumes will be brought into the room by the warm air pipe: whereas, in a room not supplied with this current of warm air, the fire will draw the air it requires either through crevices in the windows or through the door, causing cold currents of air, playing upon the persons in the room between these crevices and the fire. In this new mode of giving warm air to a room, instead of the room drawing cold air from without, it becomes filled with warm air, and is ready to expand itself through all crevices. Thus double windows, which are recommended by Count Rumford, and almost universally adopted in the north of Europe, are rendered unnecessary. There is, in this case, a universal plenum; in the other, to avoid the vacuum, the air rushes in from all parts. In a room which I have appropriated for my study, I have not had a fire for the two last years. I have a pneumatic stove, which throws in warm air; it issues from the aperture at 100 degrees up to 150, and diffuses itself so as to keep the room at the temperature of 60.

"In the common mode of warming rooms, the great difficulty is,

is, to get an equal distribution of heat. In a room, on a cold winter's day, when the thermometer in the shade stood at 28, I had as good a fire as the Bath stove, or grate of the room, would enable me to have; I then took a thermometer, and placed it in different parts of the room—in all the corners of the room where it was not under the direct influence of the fire, it stood at 45 degrees; I then brought it into the direct rays of the fire, and carried it to within a few inches of the bars, when I found it rise to 180. From this experiment we see to what a great range of temperature we are exposed, when we imagine ourselves so situated as to be free from the influence of the external air: on the contrary, in my study, there is not a variation of 5 degrees in any part of the room, except when you approach very near to the aperture at which the warm air enters. A still greater advantage of warming a study with heated air is, that the intolerable quantity of dust arising from an open fire may be avoided, and papers and books, which would otherwise be covered an inch thick with dust, remain for weeks as clean as at the time they were placed in order.

"But as we have strong prejudices to overcome, the Madeira House has fire-places in the rooms; the air which the fire will consume only adds to the greater quantity of warm air which is brought into them by the supplying tubes.

"In addition to the apartments with regulated temperature, the different suites of baths are so conveniently placed, that the occupier of these rooms will be enabled to have the use of them without being exposed to the vicissitudes of the atmosphere: thus (except in the great scheme, in which it was proposed to have attempted promenades and circuses) the inhabitant of the Madeira House will possess all the useful means of regaining lost health."

The instrument which Dr. Kentish has invented for ascertaining the soundness of the lungs is described in an appendix. The author takes for granted that a healthy man with a well formed chest is able to throw out and take in at one inspiration between eight and nine pints of air. The breathing capacity of individuals of consumptive or asthmatic habits varies from one pint to four or five. We give the doctor's description of the instrument itself:

"I have at different times used a variety of means to arrive at a knowledge of the breathing capacity of the lungs, in all different states of the subject, when in health or in disease. A bladder with a pipe attached to it will serve this purpose. If a person blows into a bladder, when he has drawn in as much air as his lungs will hold, and only makes one expiration, he may measure the quantity of air expired, by passing it under a receiver

receiver in a pneumatic trough. This mode, consisting of two operations, is liable to mistake, as the vessel the air is passed under should be sufficiently large to receive the whole of the air at once; otherwise it would require two or three operations, which would render it more doubtful. As some healthy subjects inspire above a gallon of air, the jar should hold five quarts at least, and this is above the rate of common recipients: besides, it requires some practice and dexterity to convey gaseous fluids from one vessel to another, and it is also a most unpleasant operation. These objections induced me to try other means. I then had a tin apparatus, made upon the principle of the pneumatic bellows, as described in Mr. Watts's pneumatic apparatus. The difficulty of having this so nicely balanced as to give the exact proportion, obliged me to make further efforts, for the friction of the inverted cylinder prevented the necessary sensibility of the instrument.

"The machine I use at present is a glass jar, which is inverted into a pneumatic trough; it holds two gallons of water, and is graduated by a scale, divided into pints and half-pints, unto six quarts. The trough is filled with water up to number four, so that two quarts of water stand in the bottom of the jar when it is ready for use; at the upper part of the jar a tube passes, in which is fixed a stop cork; about two inches above the cork the tube turns at a right angle, and is carried horizontally for near six inches; it is then flattened into a mouth-piece, which enables the person using the machine to close the lips about it with greater facility."

A selection of cases in which the pulmometer was applied with decided advantages closes Dr. Kentish's performance, which cannot fail to be read with interest. As his establishment increases additional improvements will no doubt continue to be made to it, and its advantages will consequently be more widely diffused, and more duly appreciated. The scheme is of course still in its infancy; but in the mean time great praise is due to Dr. Kentish for the zeal and public spirit with which he has set the establishment on foot.

LXXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 28. The conclusion of Dr. Brewster's paper on Mother of Pearl was read. The author considers the optical phænomena of mother of pearl very different from those of all other bodies: in some of his experiments the thickness of the pearl plates did not produce any change on the phænomena, in others

some difference appeared: the polarization of light by mother of pearl is different from that of the bodies which he had before examined: he found it impossible to give wax, cement, &c. exactly the same powers as Iceland spar, although they receive the impression from mother of pearl, so as to produce similar optical phænomena. It appears that the particular laminous structure of mother of pearl is such, that, however finely it may be polished, it still retains the power of reflecting highly coloured images, and that if a piece be merely cut it reflects only one image, but if polished two are seen. These facts are ascribed to the peculiar striæ of this substance, which are very fine, yet may in some specimens be seen with the naked eye; in others there are above 3000 of them in an inch.

May 5. Capt. Cater furnished a long paper, containing a description of a new instrument for dividing mathematical instruments, which he considers an improvement of Troughton's; he acknowledged that he was anticipated in the principles of his improvement, but alleged that it was never before reduced to practice. A brief description of this instrument would not be intelligible without a plate: to it he applies three microscopes, one of which is fixed and two are moveable, and suggests modes of correcting the errors of observation, and preventing the microscopes from coming in contact with each other.

May 12. Dr. B. Heyne read a paper to the Society, relating the process by which the Hindoos oxidate silver for medical purposes. The secret was communicated to him by an ancient and learned Hindoo, by whom it is esteemed of great value; he afterwards repeated the process himself. A rupee which weighs three drachms was hammered into a plate three inches broad; it was then immersed in the milk of plants, chiefly of the *euphorbia* genus. The plate was heated, and plunged into this fluid above twenty times, when it acquired only a dark-gray colour; it was afterwards placed between the leaves of plants, and repeatedly heated to a degree below smelting, and each time plunged into the milk of plants or cow-dung, till the plate was finally oxidated, so that it could be squeezed to powder between the fingers; and in this state it is given as a sovereign remedy in most diseases. Dr. H. found that water produced the same effect in facilitating the oxidation, as the milk of plants or cow-dung. As he could find no information in chemical works respecting the particular properties of the milk of plants, he was induced to make some experiments on that from several species of *euphorbia*. In some the juice is aptly called milk, like the caoutchouc or elastic gum; in others it is colourless or thin, and inspissates when exposed to the air; others again emit a deep-coloured fluid, which assumes a gummy or resinous character.

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In all these different kinds Dr. H. found traces of ammonia and azote; and hence he concludes with Sprengel, that the narcotic principle is owing to azote, which is probably combined with ammonia.

May 19. A letter to the President from Dr. Brewster was read, mentioning his observations on the effects which heated glass produces on light. Dr. B. being engaged in various experiments of this nature, noticed the effect which wax melted between two plates of glass had on light, and was thence induced to experiment on the glass properly heated. For this purpose he adopted those pieces of glass called Prince Rupert's drops; he ground them with perpendicular and parallel faces, and also at right angles; but he uniformly found that they either wholly or partially depolarized light, and that he could not transmit through them a ray of pulverized light without its undergoing some modification.

Mr. Herschel laid before the Society a mathematical paper on Analysis: it was divided into four sections; but being entirely algebraical, it was of a nature not adopted for public reading.

Captain Cater furnished an account of his additional experiments on the comparative powers of the Cassegrainian and Gregorian telescopes. His object was to discover if some error might not have crept into his former experiments, and whether he was not deceived by some optical delusion, which occasionally misleads young and sanguine observers. For this purpose he caused different persons to examine the appearances: some of them knew nothing of the subject of his inquiry, yet all of them concurred in the same observation. Nevertheless he admits the great liability to errors in such experiments, and corrects many of the observations by calculation. He made a variety of experiments with the reflectors, which in general tended to prove the superiority of the Cassegrainian over the Gregorian telescope.

May 26. Sir Everard Home, bart. gave an historical sketch of the nature and effects of the different injuries of the brain, with a view to embody facts so that inquirers and observers might more easily direct their researches. Sir Everard did not profess to state any new discovery, but merely to relate some cases which had occurred in his own practice: he divided his paper into sections, each of which describes an injury of the brain, with its physical and mental effects. The first was pressure; and if this be either too great or too little, the consequence is violent headaches, vomiting, &c. He next considered the effects of water, and related some cases where six beer pints were contained in the brain without destroying the vital functions, or the mind. In

other

other cases, a few ounces occasioned delirium, paralysis, and death. Sir Everard made several sections of the different kinds of ulceration of the brain, and detailed minutely their corporeal and mental effects. He also noticed that the cerebellum might be wounded without the patient knowing it; that part of it might be extravasated without danger, but that any wound in the pia mater was extremely painful.

The Society then adjourned to the 9th of June.

LINNEAN SOCIETY.

On Tuesday the 24th instant the Anniversary Meeting of the Linnean Society of London was held at the Society's house in Gerrard-street, Soho, for the election of a Council and Officers for the present year, when the following Members were elected of the Council, viz.

James Edward Smith, M. D.
Samuel, Lord Bishop of Carlisle.
Sir T. G. Cullum, bart.
Philip Derbishire, Esq.
Mr. James Dickson.
Aylmer Bourke Lambert, Esq.
W. E. Leach, M. D.
Alexander Macleay, Esq.
Thomas Marsham, Esq.
Wm. George Maton, M. D.
Daniel Moore, Esq.
Joseph Sabine, Esq.
Thomas Smith, Esq.
William Smith, Esq. M. P.
Edward Lord Stanley.

And the following were declared to be the Officers for the present year, viz.

James Edward Smith, M. D. President.

Samuel, Lord Bishop of Carlisle,
Wm. George Maton, M. D.
Thomas Marsham, Esq.
A. B. Lambert, Esq.
} Vice Presidents.

Thomas Marsham, Esq. Treasurer.

Alexander Macleay, Esq.
Mr. Richard Taylor,
} Secretaries.

The Members of the Society afterwards dined together at the Freemasons' Tavern, Great Queen-street, according to annual custom.

ROYAL MEDICAL SOCIETY, EDINBURGH.

The Royal Medical Society propose as the subject of their Prize Essay for the year 1815 the following question:

"The comparative specific caloric of veinous and arterial blood."

A set of books, or a medal of five guineas value, shall be given annually to the author of the best dissertation on an experimental subject proposed by the Society; for which all the Members, honorary, extraordinary, and ordinary, shall alone be invited as candidates.

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The dissertations are to be written in English, French, or Latin, and to be delivered to the Secretary on or before the first of December of the succeeding year to that in which the subjects are proposed, and the adjudication of the Prize shall take place in the last week of February following.

To each dissertation shall be prefixed a motto; and this motto is to be written on the outside of a sealed packet containing the name and address of the author. No dissertation will be received with the author's name affixed; and all dissertations, except the successful one, shall be returned, if desired, with the sealed packet unopened.

KIRWANIAN SOCIETY OF DUBLIN.

Dec. 1, 1813. A paper "On the crystallographical Method of Haüy," by Dr. J. O. Reardon, was read.

In this paper a concise statement of the theory of the learned Abbé was given, and also of the principal arguments brought forward in its support. The objections that have been offered to the system by various philosophers, as well as the replies of the Abbé, were then noticed: and a number of observations were made on the validity of the former, and the adequacy of the latter.

March 23, 1814. A paper "On an extensive Bed of Magnesian Limestone, found in the vicinity of Dublin," by S. Witter, Esq. was read.

An account of the analysis and of some peculiar circumstances attending the calcination of the stone, was first given. 36 per cent. of carbonate of magnesia were found in combination with 51 of carbonate of lime; the remaining portion being made up by silex, oxides of iron, and manganese. After some geological observations, the paper concluded with some remarks on the application of the mineral to the purposes of practical husbandry.

The same gentleman likewise read a series of observations, with an account of some experiments relating to the formation and properties of iodine. In allusion to the question of its elementary nature, he referred to some striking similarities in certain well known compounds.

LXXXV. *Intelligence and Miscellaneous Articles.*

The Annales de Chimie had been discontinued for several months in consequence of the recent events in France; but the work is now resumed. A Number bearing date in February last has just made its appearance in Paris, but a copy has not yet reached us. It contains Researches upon Coral, by M. Vogel; Experiments of M. d'Arcet on the Alloys of Platina; a Memoir of M. Vauquelin on Osmium and Iridium; Observations on phos-

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phorescent Urines, by M. Guyton Morveau; a Method to separate Osmium from Platina, by M. Laugier; a Memoir on the Boracites and *Succin*, by M. Pfaff; an Extract of the Treatise on Poisons, by M. Orfila; Annonces of New Books, &c.

POMPEII.

[Continued from p. 313.]

"The height of the walls of Pompeii may give some idea of the labours which their complete excavation requires, and which is now prosecuting with great vigour. A ditch has been excavated twelve feet broad. For the space of about eight toises the walls are completely uncovered, and persons may now walk upon the pavement of the ancient street leading from Pompeii to Nola. The other parts still remain buried. The workmen are already 500 toises from the gate at which they set out, and have cleared nearly one third of the circumference of the city. Proceeding along the great street, they have uncovered the upper part of the portico of the Grand Theatre. The point of the wall to which it adjoins is not far from the Amphitheatre. This building, although formerly partly uncovered, was again buried under rubbish because it did not present any object of sufficient importance for a museum, or to arrest curiosity. In consequence of this bad system, many houses in Pompeii were again covered with the ashes from which they had emerged. At the present moment however, and for many years back, if the excavations did not produce any thing interesting, the workmen have not desisted. They now consider every thing as new monuments added to those which they already possess. Besides, it is well known that all the tablatures, statues, and medals belonging to a cabinet, have not the same value when taken separately, but will become doubly precious when the collection is made complete.

"The excavations around the walls of the city have not suspended those in other quarters. One of the most interesting discoveries was made on the 21st of November 1812. During the preceding week the workmen had been occupied in clearing the great street leading to the Temple of Isis, and which traverses the whole of the city in a straight line. They suddenly met with another street opening into the great street, and at the joining of the two streets discovered the capitals of several columns which seemed to have composed the portico of a theatre. The excavations were then directed towards the house known by the name of *La Caza del General Championnet*, and two inscriptions scarcely legible were discovered, but appearing rather insignificant. When working about ten feet from the extremity of the street, where the rubbish consisted alternately of earth and

and ashes, and there appearing to be no probability of finding any interesting object, they were about to leave off, when they unexpectedly found a human skeleton and several bones, some medals of bronze and silver and one of gold, and finally a large heap of medals that were collected with great care. They were for the most part, particularly those of bronze and silver, fused into each other, and it was difficult to distinguish the inscriptions on account of the *patina* with which they were covered. They were medals of Domitian and other emperors, of the smallest size, very common, but well preserved; 316 in silver, and 42 in bronze. But what attracted most attention was eight beautiful medals of gold newly struck, wrapt up in several folds of linen, which seemed to have been injured by humidity and the infection from the human bodies. However, the texture was so good that these stripes could hardly be torn. This may be considered as one of the greatest curiosities which Pompeii ever afforded.

"The skeleton just mentioned was found among the ashes about ten feet above the level of the street. This is a proof of the rapidity with which the city was overwhelmed, as it is probable that this individual was endeavouring to save himself by flight. It will also make it evident that Pompeii was buried by one single and not by repeated eruptions, as some writers are disposed to insinuate.

"On the same day that this skeleton was discovered near the Theatre, several others were found in the streets. A mother flying with part of her family, consisting of two young girls and an infant, the skeleton of which was still clinging to the breast of the mother: all hopes seem to have left them; trying still to breathe amid the burning ashes, and clinging to the walls of the portico, they appear to have sunk under the effects of fatigue and grief: the lava had buried them in the same grave, and their bones were mixed with each other as if they were embracing at the last moment of their existence. Three gold rings and ear-pendents adorned with pearls, found near them, bespoke their riches and rank in society; one of the rings was in the form of a serpent in many folds. On another ring, which from its size must have belonged to a young girl, a garnet was fixed on which a thunderbolt was engraved. The ear-rings resembled those of the same age which are to be seen in the Cabinet of Antiquities at Paris; two of the pearls were in good preservation; the others have suffered considerably.

"A great quantity of marbles, adorned with forms of animals elegantly modelled, were found amassed at the foot of a part of the walls of the house where these skeletons were found, as if destined to ornament it. It seems to have been a house of the most elegant architecture, and decorated with excellent pictures, which

which for the sake of antiquarians have been permitted to remain for the present. One picture represented the figure of Peace, upon a red ground, holding an olive branch in the right hand and in the left a cornucopia: she is winged, and represented flying to diffuse her blessings over the world: a light transparent habit covers her body from the girdle downwards: on the whole, this is one of the best specimens of the style of painting of the æra in which it was executed.

"Among the antique objects found in the inside of this house, a large bronze plate was found with a double bottom, which must have served to keep the victuals hot. It resembled a similar utensil now in use, and which is occasionally filled with warm water: the difference between the ancient and modern utensil seems to be, that in the former the article to be kept hot was deposited between two thin vessels containing hot water.

"There were also a great number of glass vases found, from three to six inches deep, in the form of cups, and some drinking cups of singular appearance. They were adorned so as to represent various figures, of the different colours of silver, gold, opal, sapphire, and emeralds: time has given them a brilliancy which modern artists will in vain attempt to imitate. The glass which has been found at Pompeii is generally well wrought: the forms of the various utensils are different, but they are all regular and elegant: the bottles, caraffs, and other small vessels used in domestic affairs, are very round, and present no veins or flaws. They are mostly of coloured glass. Their utensils of white glass are by no means so beautiful as those of modern Europe.

"The same house has also produced many pieces of very curious red earth adorned with foliage, arabesques, and relievos of the finest workmanship. Some antiquarians think that they came from ancient Gaul, since many similar have been found in France. But this is erroneous, in the opinion of others, who think they are by far too elegant to have been executed in any other country than Italy."

[To be continued.]

The Class of History and Ancient Literature of the French Institute has appointed M. Vanderbourg, author of a Translation into French Verse of the Odes of Horace, and of several other literary productions, to the seat vacant by the death of M. Mercier, author of the *Tableau de Paris*.

M. Baptist Lendi, of St. Gall, has invented a new hygrometer, of which the following description is given:—In a white flint bottle is suspended a piece of metal, about the size of a hazel nut,

nut, which not only looks extremely beautiful, and thus contributes to the ornament of a room, but likewise predicts every possible change of weather 12 or 14 hours before it occurs. As soon as the metal is suspended in the bottle with water, it begins to increase in bulk, and in 10 or 12 days forms an admirable pyramid, which resembles polished brass; and it undergoes several changes, till it has attained its full dimensions. In rainy weather this pyramid is constantly covered with pearly drops of water; in case of thunder or hail, it will change to the finest red, and throw out rays: in case of wind or fog, it will appear dull and spotted; and previously to snow it will look quite muddy. If placed in a moderate temperature, it will require no other trouble than to pour out a common tumbler full of water, and to put in the same quantity of fresh. For the first few days it must not be shaken.

PROGRESS OF VACCINATION IN THE EAST.

Extract of a Letter from Bagdad, dated the 24th of August.

"Jean de Murat of Constantinople, and at present a merchant in Bagdad, has had the good fortune to introduce vaccination on the banks of the Tigris and Euphrates. Animated by a desire to be useful to his fellow-creatures in a country where small-pox, if it be not always mortal, leaves frightful mutilations and deformities, had long exerted himself to make the practice of vaccination general in this city. His humane endeavours were long fruitless, in consequence of the prejudices inseparable from ignorant minds. An accident also happened which strengthened these prejudices. The British resident eleven years ago attempted it, and failed. Seconded by Dr. Short, Mr. Jones, the English surgeon, having obtained vaccine matter from Dr. Carro and Lord Elgin, vaccinated two poor children, whom he procured more by means of money than in consequence of any assurances of success. The vaccine took in the arms of both. Mr. Jones, however, not having been careful in the choice of these two infants, one which was in good health recovered well, but the other, which had been long sickly, died a few days after being vaccinated. This accident frightened every body; and Mr. Jones has never since been able to procure any other subject for his experiments. The idea of vaccination was therefore abandoned.

"In the mean time, Jean de Murat sent repeatedly to Constantinople for matter, but all his efforts to introduce it for several years were in vain. In 1810, however, having a child born to him, he obtained an opportunity of exhibiting in his own family the excellence of this preventive of small-pox. He procured vaccine matter from Aleppo, and inoculated his new-born infant. The

The success answered his expectations, and produced the effect desired on the minds of several Christians who had been witnesses to the operation: they immediately brought their children to be vaccinated, and their example was followed by all the other Christians. The Turks and the Arabs have since been converted, notwithstanding their religious prejudices, to a firm belief of its utility. Upwards of ninety-six of the children of the latter were inoculated in the course of the year 1813; and, among others, those of the Mufti, and the Defterdar Davoud Effendi, brother-in-law to the Pacha of Bagdad, who has conferred upon Jean de Murat a public testimony of his approbation.

"Not contented with his success at Bagdad, the same benevolent Turk is endeavouring to propagate vaccination in Mesopotamia and Armenia. He has sent missionaries to Moussoul and Erivan, furnished with instructions written in Arabic and Armenian, having previously taught them to perform the operation."

M. Bucholz has recently analysed a new bitumen found in the environs of Halle in Saxony, which he thinks strongly resembles the *resino-asphaltum* described by Mr. Hatchett some years ago in the Philosophical Transactions. According to M. Bucholz, that which is found at Halle is composed of two resins, one of which is very soluble in alcohol, and approaches to the vegetable resins, forming 91 parts of the bitumen; while the other, which forms nine parts, has some analogy to amber.

The following are a few of the most characteristic marks of this substance: It is found in balls the size of an apple, enveloped in gray crystallized gypsum: in colour it is brownish, or pale yellow: fracture glossy, and very brittle: it does not become soft under the fingers; it even does not melt so easily as other resins, but while melting it exhales an agreeable smell, something like that of animal resin and styrax. M. Bucholz remarks that, as Mr. Hatchett could dissolve only 55 parts of the bitumen examined by him, while the former dissolved 91, it is extremely probable that this difference was occasioned by Mr. Hatchett's using common alcohol.

The nine grains which were insoluble in alcohol were dissolved, but with much difficulty, in boiling oil. It was fusible in a strong heat, and gave out the smell of common resin.

The 91 parts above mentioned, when separated from the alcoholic solution, were dissolved by ether, and formed a brownish tincture, while ether of a specific gravity of 0·710, rectified over muriate of lime, made scarcely any impression. Oil of turpentine and rectified petroleum have little or no effect upon this resin. Caustic potash dissolved in two parts of water does not dissolve this resin; but when the lixivium is decanted, the residue

due of the resinous principle is dissolved in water, from which we can separate the resin by the addition of muriatic acid.

The Russian Government has made considerable progress towards opening a communication with the northern regions of America by the way of Siberia. The Tschuktsches, a nation inhabiting the north-east part of Siberia, having been continually in a state of war with the Kourakes, who inhabit the shores of the sea of Ochotks, the latter threw themselves under the protection of Russia. The prudent measures adopted by the Russian Commissary Banner succeeded in inducing the Tschuktsches to make peace with the Kourakes, and to come every year into the circle of Nischnekolyma to exchange their furs for iron, tobacco, and other goods. This traffic was carried on for several years; and finally they submitted themselves to the Russian government in form. On the 9th of March 1813 they sent a deputation of 70 persons to fort Angora, on the great river Anui: these deputies took the oath of fidelity to the Emperor of Russia, and many of them were baptized according to the rites of the Greek church. The chiefs have engaged a fox's skin for every individual baptized, in name of tribute. The trade with these new subjects of the Russian empire has since become brisker than ever; and there is every reason to believe that the Russians will speedily, by advancing over-land to Behring's straits, open a communication with the people of America who inhabit these coasts, and who can supply abundance of teeth of sea-horses and furs of great value.

The new French Government has resumed the digging of the grand canal of Ourcq, in the vicinity of Paris. The works had been suspended for several years in consequence of the late unhappy state of France.

The Society for the Encouragement of Arts, &c. in France, held its grand anniversary meeting at Paris on the 11th of the present month. M. Degeraud commenced the business of the day by reading an interesting biographical notice of the late Joseph Montgolfier, the aëronaut.

Among the novelties which were exhibited, and which had been honoured by the Society's approbation, were some beautiful specimens of varnished metal fabricated by M. Deharme: various kinds of fire-arms on a new construction: some pieces of embroidered velvet of superior elegance: shawls of an extraordinary breadth (two ells and a half): various modifications of Argand's lamp: platina utensils and instruments: and porcelain vases,

vases, &c. having ornaments in relief of exquisite workmanship, intended to resemble sculpture.

Lorthier, the celebrated French medallist, died at Paris in the course of the present month at the advanced age of 82. He executed nearly all the dies of the medals and coins of France for the last fifty years.

M. Huber of Geneva last year presented to the French Institute a paper on the singular industry of a small caterpillar, to which he gives the name of *Chenille à Hamoc*, or the Hammock Caterpillar; in consequence of the manner in which it suspends itself in order to pass its dormant state, or during the period in which it remains as a chrysalis. It belongs to the family of those caterpillars which are denominated *Mineuses*; and it lives in the interior of some fruit-trees, In the month of August it ceases to eat, and then begins to spin its hammock, for the completion of which it occupies only five hours. Two cords, extended between the sides of a leaf turned inward, so as to give a concavity to the upper part, form its principal supports: it is there suspended by some fastenings of silk; and two other fastenings, which are fixed to the edges of the leaf, hold it steady as if at anchor. The hammock itself is in the shape of a small cylindrical case. M. Huber has not contented himself with simply following with attention, and describing with precision, the successive operations of this little artisan, who constructs this complicated retreat; but has endeavoured to discover how far these operations are subject to changes by the insect, and if varied according to circumstances. A worm which is taken away after it has begun the construction of its hammock, will recommence it as long as it retains silky materials sufficient for the work. If it is placed on a construction begun by another, it continues it from the point where the other has left off; but when the one to which it is placed is far advanced, it seems to prefer recommencing the whole *ab initio*. The fly which issues from the worm seems to be the *Phalæna Clerkella* of Linnæus, and one of its enemies is the *Ichneumon ramicornis*.

M. Montigre has made some curious observations on the habits and the anatomy of the *Lumbricus*, or earthworm. These animals are hermaphrodites, and all productive: and according to the observation of M. Montigre, they produce their young alive. They nevertheless require a copulation, which seems to be performed without the least intromission of parts, so that one might be led to suppose that it had no other object than

than to excite in them some orgasm necessary for the production of fecundity. This copulation generally takes place in the months of June and July, and is performed by means of an inflation, which is observable in the anterior part of the body of the worm; and these parts of different worms are, as it were, glued together; the anterior part of one worm to that of the worm opposed to it. The young ones are at first perceived in the white organs which are placed forward on both sides of the stomach, and then glide between the intestines and the exterior muscles, until they reach a reservoir situated in the thick part of the tail, where they are easily seen full of animation. M. Montigre has likewise ascertained that earthworms do not live altogether on earth, as he has found in their intestines the remains both of animals and of plants.

Theatre of Anatomy, Bartlett's Court, Holborn.—Lectures on Anatomy, Physiology, Pathology, and Surgery, by Mr. John Taunton, F.A.S. Member of the Royal College of Surgeons of London, Surgeon to the City and Finsbury Dispensaries, City of London Truss Society, &c.

In this Course of Lectures it is proposed to take a comprehensive view of the structure and œconomy of the living body, and to consider the causes, symptoms, nature, and *treatment of surgical diseases*, with the mode of performing the different surgical operations; forming a complete course of anatomical and physiological instruction for the medical or surgical student, the artist, the professional or private gentleman.

An ample field for professional edification will be afforded by the opportunity which pupils may have of attending the clinical and other practice of both the City and Finsbury Dispensaries.

The Summer Course commenced on Saturday, May 28, 1814, at Eight o'clock in the Evening *precisely*, and will be continued every Tuesday, Thursday, and Saturday, at the same hour.

Particulars may be had, on applying to Mr. Taunton, Greville Street, Hatton Garden.

LIST OF PATENTS FOR NEW INVENTIONS.

To Alexander Cock, of the Strand, in the county of Middlesex, gentleman, for his prevention and cure of the dry rot and common decay in timber, and for preserving woollen, linen, and other articles from mildew.—12th March, 1814.—2 months.

To Roger Haslewood, of Great Russel Street, Bloomsbury Square, ironmonger, for a contrivance for folding-screens adapted to impede the passage of air, smoke, fire, and light, applied to fire-places, grates, stoves, windows, and doors, which he denominates The improved Folding Screen.—12th March.—2 mo.

To

To Edward Steers, of the Inner Temple, gentleman, for a method of rendering the stoppers of bottles, jars, &c. air-tight.—12th March.—2 months.

To James Barclay and William Cuming, of Cambridge, for their improved wheels and axletrees for carriages.—12th March.—2 months.

To John Slater, of Birmingham, for his improvement in a steam boiler, and apparatus for the purpose of washing, steaming, cleansing, and whitening clothes, clothing, and cloths, and for warming or heating closets, laundries, and other rooms, by the same.—12th March.—6 months.

To Marc Isambard Brunel, of Chelsea, in the county of Middlesex, civil engineer, for a method of giving additional durability to certain descriptions of leather.—12th March.—6 months.

To Matthew Murray, of Leeds, in the county of York, engineer, for his improvements in the construction of hydraulic presses for pressing cloth and paper, and for other purposes.—12th March.—2 months.

Meteorological Observations made at Clapton and Cambridge from April 12 *to May* 10, 1814.

April 12*.—Early was *cirrocumulus* in the sky; then *cumuli*; very hot day. Thermometer rose to 72°. Barometer falling.

April 13.—Fair hot day, *cumuli* and rather hazy air; features of large *cirrocumulus* about noon. Therm. midday 73°; midnight 49°. Barom. 29.90. Wind SE, and gentle.

April 14.—Fine dry weather, *cumuli* the prevailing clouds. Wind southerly. Therm. midday 73°.

April 15.—Fine warm weather, and showers P.M. Therm. 72°.

April 16.—*Cumuli* and some *cirrus* and *cirrocumulus*; and as on former days, the *cumuli* prevail. Fine and dry. Therm. above 70°. Barom. falling. April showers from SW.

April 17.—Some showers early; fine day afterwards, and clear evening.

April 18.—Rainy morning; then showery, and finer towards evening; very warm.

April 19.—Rather windy, but warm with showers.

April 20.—Fine April weather, and showery features of the clouds. Wind westerly.

April 21.—April showers from W. Air warm.

* The dew rising from ponds remarkably white and visible this morning.

April

April 22*.—Cooler; fair morning, and chiefly cloudy. W.

April 23.—Early, features of flimsy large *cirrocumulus*, clouds through the day; cloudy evening. W.

April 24.—Mild air with showers. Wind S and SW.

April 25.—Much rain.

April 26.—Some rain in the morning; fair afternoon.

April 27.—The same weather, with gentle showers.

April 28.—Fair day, mild and warm.

April 29.—Fair; various clouds; warm.

April 30.—Fair; *cirrocumulus* and *cirrus*, with light gales as in warm weather. SW.

May 1.—Warm air, chiefly clouded, with bright intervals; *cirrus* and *cirrocumulus*; sometimes heavy clouds threatening rain. SW.

May 2.—Cloudy morning; a great deal of cloud prevailed all day; with sunny intervals; fine mild air. Wind southerly.

May 3.—Fine warm clear day, wafted by light and irregular gales and small whirlwinds as often in hot weather. *Cirrus* and *cirrocumulus* with *cumuli* forming, increasing after noon, and by evening becoming large masses; *cirrus* too remained after sunset. At midnight lofty spread clouds, with scuddy nimbiform masses sailing along lower. Wind SE.†

May 4.—Clouded day and cooler; warmer P. M. As the moon rose the clouds by degrees disappeared; the last seen were the light flimsy *cirrocumuli*.

May 5.—Cooler with a great deal of cloud and rain. Wind easterly.

May 6.—Cool morning; in the afternoon showers and much warmer. Wind SE and W.

May 7.—Clear, and cloudy intervals; much *cumulostratus*; yellow colour in the sky above, black *cumulostratus* in the west. By night, though clear, small clouds were still seen, *cirri* rapidly forming here and there, and some long elevated little *cumuli*; also *cumulostratus* barred towards one end of it at midnight.

May 8.—Somewhat cooler; fair day.

May 9.—Cold cloudy morning. Wind E and NE; very unwholesome raw air.

May 10.—The edges of the *cumuli* ill-defined, which is always a symptom of unwholesome atmosphere. Cold N wind.

Corpus Christi College, Cambridge,
May 12, 1814. THOMAS FORSTER.

* The observations following were made at Cambridge.

† This fine day brought numerous insects, particularly many of the coleopterous kind. Many of these suddenly make their first appearance and are numerous on one day. Swallows are not common here yet.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For May 1814.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
April	27	42	52	43	30·18	41	Showery
	28	43	49	45	·18	0	Fair
	29	46	55	47	·10	0	Rain
	30	47	57	45	·10	47	Fair
May	1	47	62	47	·28	52	Fair
	2	46	55	46	·02	33	Cloudy
	3	45	60	47	29·99	42	Fair
	4	44	42	42	·63	30	Cloudy
	5	40	42	45	·51	0	Rain
	6	47	52	47	·78	0	Showery
	7	48	62	46	·90	46	Fair
	8	51	57	47	30·01	35	Cloudy
	9	46	56	44	·35	30	Cloudy
	10	42	53	41	·42	46	Fair
	11	44	51	42	·42	51	Fair
	12	42	54	46	·39	36	Cloudy
	13	46	57	43	·01	0	Rain
	14	45	52	44	·01	29	Cloudy
	15	45	53	43	30·00	30	Cloudy
	16	46	56	44	·01	44	Cloudy
	17	47	60	47	·19	48	Fair
	18	52	64	46	·19	44	Fair
	19	51	60	45	·10	46	Fair
	20	53	67	45	29·95	44	Fair
	21	46	59	46	·88	47	Fair
	22	46	52	41	·62	46	Cloudy
	23	45	52	48	·53	45	Cloudy
	24	44	46	40	·50	0	Rain
	25	44	57	48	·75	33	Cloudy
	26	48	59	45	·90	47	Fair

N.B. The Barometer's height is taken at one o'clock.

LXXXVI. *Supplement to the Memoir on the Nitrates and Nitrites of Lead.* By M. Chevreul*.

Since the publication of my Memoir on the Combinations of Lead with the nitric and nitrous Acids †, there have appeared in the *Annales de Chimie* two memoirs on the same subject by M. Berzelius, being the conclusion of his experiments upon the determinate proportions in which the elements of inorganic nature exist. I purpose in the present supplement to compare our results, in order that a judgement may be formed of the differences between those of each.

M. Berzelius has described by the name of subnitrate at the minimum the salt which I have called nitrate of lead: he obtained it by putting into the solution of octohedral nitrate a quantity of ammonia insufficient to saturate its acid: I prepared it by boiling the octohedral nitrate over litharge. Our analyses differ but very little, as may be seen:

	Berzelius.		Chevreul.
Nitric acid	19·50	..	19·86
Oxide	80·50	..	80·14

They agree in proving that this salt contains twice as much base as the octohedral nitrate.

According to the observation which I have made of the correspondence between the composition of the subnitrite of lead at the minimum ‡ with the subnitrate at the *minimum* §, and from the consideration that the carbonic acid when passed into the solution of subnitrite separated from it such a quantity of base, that what remained in it was to the acid in a proportion which appeared to correspond with the composition of the octohedral nitrate ||, I suspected that there must be a subnitrate corresponding with the subnitrite of lead at the maximum; and I have even said that it would be interesting to inquire if the alkalis in acting upon the nitrate of lead would not produce it.

* *Annales de Chimie*, tome lxxxiv. p. 5.

† See the two preceding numbers of this Magazine.

‡ For the same reason that M. Berzelius has called the foregoing nitrate of lead, subnitrate at the *minimum*, he has called the subnitrite at the minimum, the salt which I have called nitrite; and subnitrite at the maximum, that which I have called subnitrite. I shall adopt this nomenclature, in order to avoid all confusion.

§ Adopting the analysis of the nitric and nitrous acids of M. Gay-Lussac.

|| I have said "*appeared* to correspond," because there is a difference of 2·78 between the result of the experiment and that of the calculation: but this difference may be owing to there remaining in the solution of the subnitrite precipitated by the carbonic acid, an excess of this acid which may concur in retaining a portion of base. If this be the case, the quantity of oxide remaining in the liquor is augmented.

This

This presumption has been verified by M. Berzelius; but the analysis which he made of this salt does not correspond exactly with the composition of the subnitrite at the *maximum*. According to this chemist, the subnitrate at the *maximum* contains:

Acid	9·81
Base	90·19

He thinks that it contains more than four times as much base as the octohedral nitrate. If, according to my analysis of the subnitrate of lead at the minimum, we admit that it only contains four times as much, we shall have the following proportions:

Acid	10·96	100
Oxide	89·04	812

and for the proportions of the subnitrite at the maximum:

Acid	9·80
Oxide	90·20

and I found by experiment the composition of this last salt to be:

Acid	9·90
Oxide	90·10

M. Berzelius and I have seen that when we boiled the octohedral nitrate of lead with lead, there was not formed any nitrate at the minimum of oxidation, but a combination of nitrous acid and of oxide of lead; that according to the duration of the ebullition, and the quantity of lead employed, we obtained two different subnitrites. We have seen besides, that a portion of the acid was reduced into nitrous gas. The agreement which subsists between these observations does not admit of there being any doubt on the subject. The same agreement seems to subsist between our analyses of the subnitrites; for the subnitrite at the *minimum* is formed:

	Berzelius.	Chevreul.
Acid and water	20	20
Oxide	80	80

The subnitrite at the maximum:

	Berzelius.	Chevreul.
Acid	10·175	9·9
Oxide	89·825	90·1

The difference is zero in the first analysis, and 0·275 in the second. Although there is this agreement, I have every reason to think that the nitrites which M. Berzelius has examined differed from those which I prepared, and this is the place to explain the results on which we differ. I premise that I do not mean to assert positively by this exposition, that I am right: I wish merely to obtain new foundations for the facts and reasonings which led me to the conclusions published in my former paper.

M. Berzelius did not find any water in the subnitrite at the maximum:

maximum: nevertheless, after having exposed it in a retort to a temperature of 100° (centigrade), as well as to the rays of the sun, I obtained water from it which I heated in an elongated tube.

M. Berzelius says that this salt crystallizes in small scales of a brick-coloured red: that which I have described was in needles of a flesh colour; and what led me to think that the salt of M. Berzelius was not so well saturated with base as mine, was, that I obtained crystals similar to those which he has described, when the solution of octohedral nitrate had not been long enough boiled over the lead: besides, in my experiment, 100 parts of octohedral nitrate dissolved 134·5 of lead, whereas in that of M. Berzelius they only dissolved 116·5: and finally, I obtained to the very last needles of flesh colour from the solution of nitrate boiled over lead, whereas M. Berzelius obtained subnitrite at the minimum with his subnitrite at the maximum.

M. Berzelius prepared the subnitrite at the minimum by M. Proust's process; but, according to mine, the salt thus prepared is not a pure nitrite: it contains nitrate of lead. I think that my experiments ought to leave no doubt on the subject: for (*a*) when we dissolve this salt in water, we obtain subnitrate at the minimum; (*b*) when we treat its solution by the carbonic acid, we separate from it a part of the oxide, and by evaporating the liquor we obtain, 1st, *scales of a yellowish white*, formed of subnitrite and subnitrate at the minimum; 2d, *white needles of subnitrate at the minimum;* 3d, *yellow crystals, resembling in form the octohedral nitrate of lead.* (*c*) When we decompose the subnitrite at the maximum by the carbonic acid, and evaporate the solution in the sand-bath, there are separated yellow crystals of subnitrite at the minimum*, which being redissolved in water yield only yellow scales of subnitrite at the minimum, whether we concentrate the solution, or treat it by the carbonic acid, and afterwards evaporate it in the sand-bath. Now if the nitrate of lead which we get from M. Proust's nitrite is formed during the treatment given to this salt, why should it not be formed, also, when we submit to the same treatment the subnitrite at the minimum coming from the subnitrite at the maximum?

M. Berzelius, in treating the subnitrite at the minimum with a quantity of sulphuric acid sufficient to separate from it the half of the oxide, obtained *octohedral crystals of a citron yellow* which he regarded as neutral nitrite. It is evident that these crystals are the same with those which I have extracted from

* Notwithstanding all this, there may be a little nitrate of lead in these crystals: I have even tried to account for their formation. See the note to No. 43 of my memoir. I have had no other evidence, however, of this formation, except the pale colour assumed by the scales in some operations.

 the

the subnitrite at the minimum of M. Proust, decomposed by the carbonic acid. M. Berzelius in treating them with water obtained subnitrate from them. He thinks that this last is formed during the operation; but what I have reported above seems to contradict this opinion. These crystals, according to this chemist, give 0·70 of residue when we distil them. I have said that they may be formed of acid nitrate and of nitrite: the result of M. Berzelius supports this conjecture. If there exists a neutral nitrite, we ought to find it in the solution of pure subnitrite at the minimum passed over carbonic acid and left to itself. See the note to No. 37 in my memoir.

M. Berzelius says that the white pellicle which is formed by the contact of the air in the solution of the subnitrates is subnitrate: I regard it as being produced by the combination of the carbonic acid of the air with the oxide of lead. I found my opinion upon the subnitrite at the minimum, when dissolved in water, not giving any white pellicle when it is in contact with pure oxygen gas, and when it is disturbed by a single atom of carbonic acid: I do not pretend, in short, that the carbonate of lead which is precipitated is not mixed with a little subnitrate: I have not made any experiment to enable me to ascertain the contrary.

M. Berzelius, having seen that the subnitrite at the minimum left 0·80 of base when it was distilled, and that the greatest part of the acid disengaged was condensed in the water of crystallization in the state of fuming nitric acid, endeavoured to determine, according to the laws which he had discovered, the relation of the water to the acid. He determined this ratio on the hypothesis that azote is a simple body, and also that it is a compound of oxygen and ammonium. On the first hypothesis, the subnitrite at the minimum ought to be formed:

Acid	18·13
Base	80·00
Water	1·87

But according to M. Berzelius, this result is inadmissible, because the water is in too small quantity to enable it to condense the greater part of the acid to the liquid state, and this quantity does not agree with the law of the water of crystallization of salts. According to the second hypothesis, the subnitrite ought to be formed:

Acid	13·6
Oxide	80·0
Water	6·4

M. Berzelius admits this result, because it agrees with the laws which he has established: thus the base contains 5·72 oxygen, water 5·88, and the nitrous acid twice as much.

I have

I have spoken in my memoir of the difficulty of determining the water of the subnitrite at the minimum, because the acid begins to be disengaged from it at a temperature of 100° centigrade: in order to attain this, I heated on a sand-bath at a gentle heat some subnitrite at the minimum, until no more diminution of weight took place. The salt when thus heated still retained some water. As it may be objected with reason that a portion of acid may have been volatilized, I made a test, by distilling the subnitrite with its water in a small glass retort communicating with a tube filled with muriate of lime: the result of this experiment confirmed the former; only the proportion of base was a little stronger. It would be needless to detail this experiment; but, as M. Berzelius is of a different opinion, I shall describe it minutely.

I put 100 parts of pure subnitrite at the minimum which contained 80 of base into a very small glass retort blown with a lamp. A tube 14 millimetres in diameter, curved, communicated with the retort by one of its extremities, and by the other to a bent tube filled with calcined muriate of lime. The extremities of this tube by which the gases were extricated were inserted into mercury. The weight of the whole apparatus was determined with a very nice balance. I put the retort on a wire grating supported by bricks, and placed under it a spirit lamp: when the retort was heated, I surrounded it with burning coals: a little water was extricated, nitrous acid gas in great quantity, which soon filled the whole apparatus, and a part of which escaped. When the operation was terminated, and when all the liquid which moistened the sides of the retort had passed into the intermediary tube, I stopped the operation: I closed this tube, and that containing the muriate of lime with stoppers previously weighed; I introduced a tube into the retort, and blew into it, in order to drive away the nitrous vapour: without this, the latter would have been absorbed when the oxide of lead was cooled.

When the retort was weighed, I passed joseph paper into the neck, in order to take up the water which might be there. I weighed it again, and found that it had not perceptibly diminished in weight: there remained in the retort 82·5 of oxide, while the same salt decomposed in a crucible had given 80. I ascribe this difference to a portion of the acid not having been disengaged by heat; or, in spite of the precaution which I had observed of blowing into the retort, a portion of the vapour had already entered into combination. The intermediary tube contained 4·6 parts of water, and the tube with the muriate of lime 0·5, which makes for 100 parts of subnitrite at the minimum 5·1 of water. But this water was saturated with nitrous acid,

and the tubes which had been weighed full of air, made it full of nitrous acid gas: hence the subnitrite at the minimum cannot contain 6·4 of pure water, as M. Berzelius says. I have said that this result confirmed that of my analysis; for, according to the latter, there must have been more than 2·26 of water in 100 of subnitrite at the minimum: now we can easily conceive that the latter had been able to absorb nearly its weight of nitrous and nitric acid. I shall now detail another method of determining the water of crystallization of the subnitrite at the minimum, and it is entitled to the more confidence as I shall deduce it from experiments described in my memoir.

When we pass into a solution at the maximum a stream of carbonic acid, we precipitate from it such a portion of oxide that the portion which is not precipitated is to the acid in the relation of 72·48 to 27·52: this ratio seems constant; for I have shown that the quantity of water in which the subnitrite was dissolved had no sensible influence on the result. The decomposition is stopped by the presence of an excess of nitrous acid: now the subnitrite at the minimum being partly decomposed by the carbonic acid, it is evident that the two decompositions ought to expose a quantity of nitrous acid, which ought to be to the oxide not precipitated in the same ratio. This being the case, we may know the quantity of acid contained in the subnitrite at the minimum, if we have determined the proportion of the elements of the subnitrite at the maximum, and the quantity of the base of the subnitrite at the minimum. Now M. Berzelius and I are agreed as to these quantities: we admit 80 as the base in the subnitrite at the minimum: several experiments which I made on the decomposition of the subnitrite at the minimum proved to me that the carbonic acid separated from five grammes of this salt 1·74 gr. oxide, of which there remained in the liquor 2·26 of base: now when we establish this proportion, 72·48 : 27·52 : : 2·26 : x, we shall have 0·858 of nitrous acid contained in five gr. of subnitrite at the minimum. Now if we subtract from 5 gr. 4 gr. of base and 0·858 of acid, there will remain 0·142 for the water of crystallization, or for 100

Acid	17·16	17·67	100
Oxide	80·00	82·33	465
Water	2·84		

According to the first determination, I had admitted a little more than 2·26 of water. If we now calculate the proportion of base contained in the subnitrite at the maximum, supposing that it is double that of the subnitrite at the minimum, we shall have 90·2 instead of 90·1, which I found by experience.

LXXXVII. *On*

LXXXVII. *On Errors in the Nautical Almanac.*

June 1, 1814.

SIRS,—BEING much struck with the notices of the numerous and remarkable errors in the Nautical Almanac for the year 1816, mentioned in your last number (193), on immediately turning to the work, I found that all those errors, though really existing, are not to be found in the several months in the body of the work, but are all contained in what may be considered the 2d page after the preface, titled *Principal Articles*, &c. where are found not only all the said errors mentioned by Criticus, but many others also, which he has passed over in his haste; for I find that all the twelve Ember days, mentioned in the said page of the Almanac, are every one of them wrong. Thus,

What are said to be Feb. 19, 21, 22, ought to be March 6, 8, 9;
and instead of May 20, 22, 23, ought to be June 5, 7, 8;
and instead of Sept. 16, 18, 19, ought to be Sept. 18, 20, 21;
and instead of Dec. 16, 18, 19, ought to be Dec. 18, 20, 21.

It then occurred that possibly similar errors might exist in some of the other year's Almanacs; and accordingly, in turning over that for 1815, many errors soon appeared. Thus, in the column of Holidays, &c. in the month of May, page 49, there are two, viz.

Oxford Term ends on the 11th, instead of the 13th day;
and *Oxford Term begins* on the 24th, instead of the 20th.

Again, in the like column of June, page 61, we find

Trinity Term begins on the 3d, which was on May 24.
Trinity Term ends on the 21st, instead of the 14th.
On the 5th day, in 8 days 2d return, for in 15 days 3d return.
On the 12th day, for 15 days 3d ret. read 3 weeks 4th return.

Again, in July, page 73, in the like column, all the numbers of the Sundays are wrong, being all 1 too many; so that instead of 6th, 7th, 8th, 9th, and 10th, should be the 5th, 6th, 7th, 8th, and 9th.

The same mistake happens in the month of August, where the 11th, 12th, 13th, 14th Sundays should be the 10th, 11th, 12th, 13th.

Under the article *Other Phænomena*, in the last column of the first page of every month, many things, particularly the Occultations, are omitted, which it was usual formerly to insert. There are several other smaller omissions in different places, besides one of more remarkable import entirely omitted; viz. the Transit of the planet Mercury over the Sun, which will take place on the 12th of November that year.

Such discoveries as these naturally create a suspicion as to the

the accuracy of other parts of the work, if time would permit the investigation.

I am, sirs,

Your obedient servant;

To Messrs. Nicholson and Tilloch. CRITICUS SECUNDUS.

LXXXVIII. *New Inquiries into the Nature of the Liquor obtained by the reciprocal Action of Sulphur and Charcoal. By M.* CLUZEL*. *Read before the Institute.*

IN 1796, M. Lampadius having had occasion to distil iron pyrites with charcoal, in order to extract more sulphur than by the common process, obtained a liquor to which he gave the name of alcohol of sulphur, and which he regarded as being formed of sulphur and hydrogen. Messrs. Clement and Desormes some years afterwards obtained the same liquor by passing sulphur into a porcelain tube made red hot through charcoal previously calcined: they submitted it to a great number of experiments, which induced them to regard it as carburetted sulphur; they thought that this liquor resulted from the combination of one-third in weight of charcoal and two-thirds sulphur: they admit of no hydrogen. Subsequently, M. Amadée Berthollet, in a very excellent paper inserted in the first volume of the *Mémoires d'Arcueil*, has communicated a series of researches on this same liquor, which led him to conclude that it did not contain an atom of charcoal, and that it resulted from the union of sulphur and hydrogen in variable proportions, but always, with respect to hydrogen, less considerable than in the sulphuretted hydrogen gas, and stronger than in the hydrogenated sulphur obtained from the hydrogenated sulphurets by M. Berthollet's process. M. Vauquelin at the same time made inquiries into those bodies, which led him to consider them also as hydrogenated sulphur.

Struck with the variety of opinions of chemists so eminent, and whose observations merit so much confidence, I thought that this liquor must be variable in its composition, and that probably some circumstances had hitherto escaped the authors to whom I have alluded. I thought that it would be interesting for science to undertake once more some fresh inquiries respecting this singular liquor. Circumstances prevented me from carrying my labours so far as I could have wished, although I made a great number of experiments; but I am of opinion that the facts which I have had occasion to observe will be agreeable to the class.

* *Annales de Chimie,* tome lxxxiv. p. 72.

My

My first care was to procure a sufficient quantity of this liquor. The process of Messrs. Clement and Desormes requiring the use of porcelain tubes, and consequently an expensive apparatus, I endeavoured to procure a sulphuret which should easily give out its sulphur, and I thought that none would better serve, the purpose than the sulphuret of iron at the maximum: viz. iron pyrites. M. Lampadius only succeeded once in obtaining it by this process, but the cause must have been owing to something which I proposed to discover.

Operation I. I took one kilogramme of pyrites from the *Pas-de-Calais*, clean and pure, and 500 grammes of common charcoal, light, brilliant and sonorous. These two substances, when reduced to a very fine powder and passed through a hair sieve, were perfectly mixed, and introduced into a stone retort. To the neck of the latter was fitted a lengthening tube entering a bell-glass with three necks (*tubulures*): one of them entered a very dry flask, and the other a bell-glass with two tubulures to which was adapted a tube thrice curved, the extremity of which entered into an earthen pot full of water. The whole being perfectly well luted with fat luting, and the first joint covered with linen smeared with white of eggs and lime, a fire was kindled under the retort, which was made red-hot, and towards the end of the operation the dome of the furnace was surmounted by a funnel or vent. At first, before the retort became red, there was extricated a small quantity of a diaphanous liquid which had all the appearance of water. Afterwards there came over a considerable quantity of gas. Several flasks of it were collected at various stages of the operation, care being taken always to use the same water contained in the earthenware jar, in order to fill the flasks. This extrication of gas lasted during the whole of the operation. The two bell-glasses were covered with cloths, which were wetted with cold water from time to time. The liquor, which was at first diaphanous, became milky in about an hour. After four or five hours of heat carried the length of actual incandescence, the disengagement of the gas having stopped, and nothing more passing over into the receiver, the operation was stopped, the earthen jar removed, the extremity of the tube well dried with joseph paper, the aperture was well closed with fat luting to prevent the access of air, and after the apparatus was cooled the matters which it contained were examined.

Three products were obtained by this operation:

1. Gases in a very great quantity, which were ascertained to be a mixture of sulphuretted hydrogen, part soluble and part insoluble in water, oxycarburetted hydrogen, carbonic acid and azote, in proportions variable according to the stage of the operation.

2. A

2. A milky liquid, weighing 43·5 grammes, apparently water saturated with sulphuretted hydrogen and mixed with precipitated sulphur, probably from the reaction of a small quantity of sulphurous acid on a portion of sulphuretted hydrogen.

3. A very slight layer of a solid, flexible, lamellous reddish matter, having a slight smell of hydrogenated sulphur, and lining the inside of the two bell-glasses.

Operation II. The substance remaining in the retort, which had been carefully closed, was withdrawn: immediately upon taking away the lengthening tube, one kilogramme was added of the same sulphuret of iron pulverized and well dried in a melting-pot. It was mixed perfectly and very speedily, in order that the matter might be as little as possible in contact with the air. The mixture was introduced into a retort, the apparatus above described was adapted, and the operation conducted in the same way.

Much less gas was set free: several flasks were collected at various stages of the operation, always using the same water. These gases were, as in the first operation, a mixture of sulphuretted hydrogen, carbonic acid, oxycarburetted hydrogen and azote in variable proportions. There was remarked, as in the first operation, a liquor at first diaphanous, afterwards milky, and then slightly citron-coloured. The operation was stopped in four or five hours, and the apparatus dismounted. In the flask there were 30·4 grammes of a liquor composed of two layers: the upper was slightly yellow, the under slightly citron.

The lengthening tube contained a much thicker coating of reddish brown matter, flexible, lamellous, and slightly elastic.

Operation III. There was added as above 1 kilogramme of the same sulphuret of iron pulverized and dried, but only one half of the residue of the preceding operation, and the operation was conducted in the same way. The same phænomena were observed, but the gas was less considerable; and it was suffocating, having the smell of sulphureous acid at the same time with that of sulphuretted hydrogen. This smell had not been remarked in the first two operations, and we were far from expecting it. M. Proust, however, in his experiments upon the metallic sulphurets* had remarked a similar circumstance. We shall see a little further on, under what circumstances these two gases may be in contact without being decomposed. The water in the earthen jar, which in the first two operations had remained nearly transparent, was milky on this occasion.

Operation IV. Two kilogrammes of the same sulphuret of iron were added to the half of the residue of the preceding operation. 72·5 grammes of liquor formed like the former were obtained.

* *Journal de Physique,* tome liii. p. 90.

obtained. The lengthening tube contained a greater quantity of reddish-brown matter.

Operation V. To the residue from the preceding operation was added 1·500 kilogramme of the same substance. 8·3 grammes of a milky liquor resulted from this operation.

*Examination of the ethereated liquor**. Overlooking the products of the first and fifth operations, which were merely water saturated, the former with sulphuretted hydrogen, and the latter with sulphurous acid, I collected the liquors of the second, third, and fourth operations, which seemed to be identically the same. They were introduced under distilled water into a small glass retort, the beak of which entered into the neck of a tubulated matrass filled with distilled water up to the bottom of the neck, so as to make the gasified matter arrive immediately into the water, without the latter being able to reascend into the retort. To the tubulure of the matrass was fitted a tube for collecting the gases. The whole being closely luted, heat was applied, and 120 grammes of a limpid and volatile colourless liquor were obtained. Its specific gravity was obtained by filling a bottle with a ground stopper, which weighed, the temperature being 20°, 100·357 grammes; when full of distilled water, 167·712 grammes; and when full of the liquor, 185·423: hence it weighed 1·263. Its taste was acrid and caustic; smell fetid, very pungent, and not at all resembling that of sulphuretted hydrogen: it is in short a smell *per se*.

Its elasticity (*tension*) is very great: it was found, as will be afterwards seen, from 0·3184 metre to 0·7527 metre of pressure, and 22° 5′ of temperature.

It is not very soluble in water, but very soluble in alcohol. This solution when poured into the water rendered it milky, like the volatile oils and resins precipitated by water from their alcoholic solutions. M. Vauquelin thought that this precipitate was sulphur in a very minute state of division †; but I have ascertained it to be the liquor itself, apparently not altered, and abandoned by the alcohol, which unites in preference with the water. This liquor being in a state of extreme division, and being but slightly soluble in water, affects its transparency, and gives it a milky appearance, analogous in fact to that produced by sulphur slightly hydrogenated, precipitated from the sulphuretted hydrosulphurets by an acid: but this cannot from the nature of things be sulphur. When we pour this alcoholic solution into a great quantity of water, the turbidness which is at first formed disappears entirely, on account of the solubility of

* Thus I denominate the liquor obtained from pyrites calcined with charcoal, or directly from sulphur and charcoal when exposed to a high temperature.

† *Annales de Chimie*, tome lxi. p. 151.

the

the liquor: if it were sulphur, this could not take place. Besides, when we do not use too much water, we see the milky liquid gradually become transparent, and drops of liquor are collected at the bottom of the vessel; they have the same appearance, the same transparency, volatility, taste, and in short the same properties in every respect above enumerated.

Liquid sulphurous acid effects no change in it: two grammes of liquor kept in contact for about two months, and frequently agitated with 60 grammes of concentrated sulphuric acid, exhibited no alteration.

The sulphurous acid gas causes no kind of alteration, as we may be assured by placing some in contact with azotic gas saturated with liquor.

It burns without leaving any charry residue. Four grammes were burnt in a porcelain saucer: no traces of charcoal were discovered, but merely a little sulphur. The flame was white, violet at the extremity, and shaded now and then with red: it gave out a very strong smell of sulphurous acid.

When it was volatilized out of the contact of the air, it left (although it was absolutely colourless and limpid) a small black residue which might be taken for charcoal, but in such a small quantity that it would be difficult to decide upon it: there are only some distinct traces of it when we act upon 17 grammes.

The water of the matrass of the apparatus above described for the rectification of this liquor was colourless, diaphanous, and had a peculiar smell which seemed to be that of the ethereated liquor very dilute, and not that of sulphuretted hydrogen: it was water, holding in solution a very small quantity of liquor. The water contained in the retort was transparent, and had the same smell, but was slightly citron-coloured. At the bottom of the retort was a yellow, friable and crystalline matter, which seemed to be nothing but sulphur. There was 15·3 grammes.

M. Amadée Berthollet inferred the presence of hydrogen in this liquor from very strong analogies. I demonstrated this fact in an incontestable manner by isolating the hydrogen by means of a body which seizes upon the sulphur—iron or copper for instance—and the latter in preference, because it does not possess the property of combining with the carbon, which I hoped to isolate at the same time, if the liquor contained any. The following is the apparatus I used:

I took 20·13 grammes of very fine copper wire, and introduced it coiled up into a glass tube luted externally. This tube passed through a furnace. To one of the extremities was fitted a small glass retort containing ten grammes of ethereated liquor: to the

the other extremity was fitted a very small lengthening tube communicating with a very small tubulated matrass, furnished with a tube for collecting the gases, the extremity of which was inserted into water. The whole apparatus had previously been most carefully dried; and the whole being perfectly luted, the retort was heated a little to drive off all the air contained in the retort and in the tube. The luted tube was then gradually heated: scarcely had it become red, when there was a considerable explosion. The matrass and lengthening tube were driven a great distance, and broken to atoms. The tube appearing to be untouched, there was substituted, after every thing was cooled, another lengthening tube, and another small matrass fitted with a tube to collect the gases. The whole being well luted, the retort was heated longer than at first, on the idea that the explosion was owing to a small quantity of air which remained in the apparatus: the luted tube was made red hot, and the retort slightly heated. No gas was emitted. All the liquor having disappeared, the heat was gradually slackened, and I sealed hermetically with fat luting the extremity of the bent tube, after having dried it well with bibulous matter to prevent the admission of water.

Next day the apparatus was taken to pieces: the tube was in part fused, but there was no fissure in it. When broken, there was found among the broken glass the copper, which was very brittle, and covered with a black, brilliant, light matter having the appearance of charcoal. It weighed 22·15 gr.: it had therefore augmented in weight 2·02 grammes: there was besides a little matter disseminated and incrusted in the melted glass. There were found in the matrass two or three grammes of a pink liquor of an acid taste, and so volatile that a few drops only could be collected. There remained in the retort a small black, solid residue, adhering strongly to the side of the retort, and in so small a quantity that it was impossible to examine it.

Since no gas was extricated during this operation, if the liquor subjected to the experiment contained hydrogen, it must have been found in the new red-coloured liquor, which would account extremely well for its volatility, since the copper seems to have taken from it sulphur, and to have isolated from it a portion of charcoal. To ascertain this, I made the following experiment:

The copper, which had become brittle, and covered with a blackish charry matter, was bruised in an agate mortar: the pieces of copper which were still ductile were separated from it, and about one part of pure crystallized nitre was added to it: the whole being pulverized and mixed, it was projected from the burning charcoal, and far from the furnace, into a red-hot Hessian crucible very clean. The deflagration was very prompt. Afterwards we treated with distilled water, filtered speedily, and precipitated

precipitated the liquor by lime water in a long and narrow bottle. There was an abundant and flaky precipitate. The bottle was corked and luted, and next day the water was decanted. The precipitate was in a heap, and adhering to the bottom of the bottle: it was filled with mercury, and turned upside down in the trough; acetic acid was introduced, diluted with about an equal part of water. A brisk effervescence took place. The gas disengaged was passed into a smaller bottle, and it was several times passed from one bottle to another through water, in order to wash it, and free it from the little acetic acid with which it might have been mixed. This gas was without any sensible smell: it extinguished combustion, reddened turnsole tincture, and precipitated lime water. It was not very soluble in water, but nevertheless entirely absorbable by it: it was carbonic acid. There was charcoal, therefore, in the ethereated liquor. We cannot suppose that this carbonic acid was owing to carbon foreign to the liquor: every precaution had been taken to remove every cause of error. In short, we shall see other proofs of this in the course of this paper. The liquor from which the carbonic acid had been precipitated by the lime water was precipitated by the water of barytes. The precipitate formed was sulphate of barytes.

The blackish matter found in the tube after the operation was therefore sulphuret of copper mixed with charcoal. At least it is not probable that these three bodies are united: and if we may be permitted to refer it to the appearance, we might even affirm that the carbon was deposited on the surface of the sulphuret of copper, which seemed really to have been strewed with it.

[To be continued.]

LXXXIX. *Experiments on the variable Action of the Electric Column.*

London, June 13, 1814.

Sirs,—I have the pleasure to inclose you an account of some experiments on the action of the electric column, communicated to me by an electrician of great promise, whose scrupulous attention to the essentials of accurate experimental inquiry it has frequently afforded me pleasure to observe.

It will be seen that these experiments render the conclusion drawn by Mr. Howldy, from the ingenious experiments related at page 241, vol. xliii. of your Magazine, rather exceptionable; and that they tend to confirm the opinion I have advanced in the "Elements of Electricity," page 478, that variations of temperature have the most evident influence of any known cause on the action of the electric column.

I have

I have commenced a series of experiments on the effects of heat applied to various Voltaic combinations, which may possibly present an answer to the question proposed in this paper by Mr. Ronalds: but, from the necessity of constructing new apparatus for this purpose, it may be some time ere I am able to publish their results. I am, sirs, yours, &c.

To Messrs. Nicholson and Tilloch. G. J. SINGER.

Experiments on the variable Action of the Electric Column. By FRANCIS RONALDS, *Esq. Communicated by Mr.* SINGER.

Hammersmith, June 10, 1814.

VERY soon after I had constructed a pendulum apparatus similar in principle to that of M. De Luc described in vol. xxvii. page 161 of Nicholson's Journal, and had observed the variety of its action, I suspended an hygrometer and a thermometer near it, by which I found that when the latter rose but one degree, a difference of four to five vibrations in five minutes took place, whilst no such effect occurred if the former advanced two or three degrees towards dryness unaccompanied by a corresponding rise of the thermometer. It was therefore evident that heat is the principal cause of the variable action of the electric column.

The following experiments were made with a gold-leaf electrometer, in order that they might be repeated and varied by any one who may not be inclined to construct the pendulum apparatus of M. De Luc's contrivance, or provide himself with one.

The negative end of a column consisting of 1000 groups of zinc and gold paper, inclosed in a glass tube with interposed discs of paper as improved by Mr. Singer, was screwed upon one side of a brass tripod, an electrometer on another side, and an hygrometer and thermometer on the third side. A glass dish was placed under the tripod, and the whole apparatus arranged upon the plate of an air pump.

An observation was generally made every five minutes. In the 3d Experiment the mean of several observations is set down instead of each, for the sake of brevity. The first columns give the time; the 2d, the degree of the hygrometer; the 3d, that of the thermometer; and the 4th gives the number of strikings per minute of the gold leaves.

Experiment 1. *in open Air.*

Time. Hour.	Min.	Hygrom.	Therm.	Strik.
3		41	53½	5
3	5	41½	Idem	Idem
3	10	Idem	Id	Id
3	15	Id	Id	Id
3	45	41	54	Id
4		Id		Id

Experiment 2. *in the Receiver.*

Time. Hour.	Min.	Hygrom.	Therm.	Strik.
4	5	41	53½	5
4	10	Idem	Idem	Idem
4	15	Id	Id	Id
4	30	Id	Id	Id
6	15	41½	53	3

Experiment 3. Room gradually heated.

Time. Hour. Min.	Hygrom.	Therm.	Strik.
6 30 to 6 45	41·12	53·75	3·75
6 50 to 7	40·33	55·33	5 66
7 5 to 7 15	39·5	57·5	6·5
7 20 to 7 40	38·28	58·8	7·4
7 45 to 8	37·5	61·18	8·
8 5 to 8 30	36·83	63·38	8·66
8 35 to 8 45	35·66	65·33	8·66
8 50 to 9 5	35 22	66·25	10·5
9 10 to 9 25	34·5	67 25	12 5
9 30 to 9 50	33 25	68·5	13·5
9 50	32·	70·	17·
9 55 to 10	31·5	71·	leaves stuck to the sides frequently.

Experiment 4. Receiver removed.

Time. Hour. Min.	Hygrom.	Therm.	Strik.
0 20	40·5	52	2·5
25	Idem	Idem	5·3
30	Id	Id	Idem
35	Id	Id	Id
40	Id	Id	Id
45	Id	Id	Id

Experiment 5. Sulphuric Acid placed in the Dish.

Time. Hour. Min.	Hygrom.	Therm.	Strik.
55	37	52	2
1 0	31	Id	Id
1 5	29	Id	Id
1 10	25	52·5	2·5
1 15	21	Id	3
1 20	19	Id	2 5
1 25	18	Id	3
1 30	16	Id	Id
2 15	11	53	Id
2 30	10	53·25	3·5
2 45	9·5	53·5	3
3 0	9	54	3·5
3 15	8 5	Id	5
3 30	8	Id	4·5
3 45	Id	54·25	5
4 0	7·5	Id	Id
4 15	Id	Id	Id
4 30	Id	54·5	Id
4 45	7	Id	Id
5 55	6 5	54·25	4

Experiment 6. Receiver and Acid removed.

Time. Hour. Min.	Hygrom.	Therm.	Strik.
6 5	25	55 5	6
6 20	36	Id	Id
7 0	37	Id	Id

Experiment 7. Water placed in the Dish, and Receiver replaced.

Time. Hour. Min.	Hygrom.	Therm.	Strik.
7 15	39	55	4·5
7 20	43	54·5	Id
7 25	48	Id	Id
7 35	63	Id	3 5
7 40	66	Id	Id
7 45	69	Id	3
7 50	71	Id	Id
7 55	74	Id	2·5
8 0	75	Id	Id
8 5	76·5	Id	2
8 10	78	55	Id
8 15	79·5	Id	Id
8 20	80 5	55·5	3
8 25	81	55·25	2·5
8 30	82	Id	Id
8 35	82·5	Id	Id
8 40	83	Id	2
8 45	83 5	Id	Id
8 50	84	Id	Id
8 55	84·5	Id	1·5
9 0	85	Id	Id
9 5	85·5	Id	Id
9 10	86	Id	Id
9 15	86·5	Id	1
9 20	87	Id	0·5
9 25	87·5	Id	0·3
9 30	88	Id	0 28
9 35	88·5	Id	Id
9 40	89	Id	0 25
9 45	89·5	Id	0
9 55	90	Id	0
10 0	90·5	55·25	1 in. div.
10 10	91	Id	0 7 do.
10 20	91·5	Id	0·6 do.
10 30	92	Id	Id
10 40	92·5	55	Id
10 50	93	Id	Id
11 55	95	53	Id

Experiment 1. Was destined to try whether at equal degrees of heat and moisture in the open air an uniform action prevailed, and the result shows that it was very nearly so.

Experiment 2. Shows that the same uniformity continued in the receiver, which was not air-tight.

Experiment 3. Shows that when the air of the receiver was gradually heated from 53 to 70, and by this means the hygrometer made to advance $9\frac{1}{2}$ degrees towards dryness, the electrometer increased its strikings progressively from 3·75 to 17 times per minute.

Experiment 4. Proves that the column had not undergone a permanent change by Experiment 3.

Experiment 5. Shows that when the air of the receiver was dried by means of sulphuric acid from 37 to $6\frac{1}{2}$, giving for the whole diminution of moisture $30\frac{1}{2}$ degrees, no increased intensity took place.

Experiment 6. Proves that the column had not been permanently affected by the last Experiment.

Experiment 7. Shows that when the the air was moistened 56 degrees, the intensity was diminished from $4\frac{1}{2}$ strikings per minute to a divergence of $\frac{6}{10}$ inch.

It would be unnecessary and tedious to detail some other Experiments which were made with a different view, but which conspired with the above to indicate that heat is the principal cause of the phænomenon, and that a moist atmosphere produces exactly the same effect on the glass tube of the column that it does on the insulators in all other electrical experiments.

It now therefore seems a matter of some interest to resolve several queries relative to the mode in which heat acts: for instance, Does it promote the electromotive powers of the metals? Does it produce the effect by giving motion to the combined moisture of the interposed disks of paper? or by rendering those disks more or less conducting? Is there any analogy between the column and the tourmalin? The justly celebrated inventor of this modification of the Voltaic pile, having first observed that equal degrees of heat and moisture were not accompanied at different times with a corresponding intensity of action, conceived that this difference was occasioned by changes in the electrical state of the ambient air, for which reason he gave it the name of Aërial Electroscope, and I think the evidence of our present experience preponderates in favour of the conjecture.

Beccaria, Read, and others have shown that the state of moisture of the higher regions of the atmosphere is intimately connected with that of its electricity: a height of only six or eight feet above the surface of the earth is even sometimes sufficient for collecting powerful electric signs in an open situation.

 I have

I have by means of a long wire insulated on Mr. Singer's plan, when a copious dew was falling, collected pungent sparks when the height of the wire did not exceed five feet above the surface of the earth in any part of it. Hence it is concluded, that we cannot measure the electricity of the higher strata of the atmosphere until that of the lower, which may be excited by the evaporation or condensation of moisture, or by position, has been first measured. Now, if this electricity of the lower strata could be proved to exist in a very small degree in closer situations, there would be no reason why it should not influence the column.

Mr. Read has in my opinion done much towards this, by the aid of the doubler.

The signs which the doubler produced were certainly strongly analogous to those of his rod, and there are several other experiments which appear to me to prove that what is called the Adhesive Electricity of such-like instruments is occasionally no other than that of the ambient air, which their peculiar structure is calculated to collect and display.

P. S.—Since writing the above, I have shown the tables to Mr. Singer, and was glad to find that he not only coincided in the conclusions drawn from them, but also in the idea that the electric state of the ambient air may be sufficiently powerful to influence the column: but I wished to ascertain the effect of heat upon a column not inclosed in a tube, as it might be objected, that the air immediately in contact with the pile I used, and the inner surface, was not dried by the acid, although the hygrometric equilibrium might have been varied by heat. I therefore borrowed from Mr. Singer a pile consisting of 800 groups of zinc, silver, and paper supported between three pillars of glass covered with sealing-wax, and placed it in a very large receiver, together with an hygrometer, thermometer, and electrometer: the receiver was now placed over mercury.

The divergence of the gold-leaves was estimated as nearly as I could guess by the eye, and the following results were obtained:

	Time.	Hygrom.	Therm.	Electrom.
June 2d.	11 40 A.M.	45	56½	divergence 1·5 inch
	3 0 —	42	76½	struck 3 times per minute
	10 0 —	45	64	1·2 — per minute
June 3d.	1 0 P.M.	44	58	divergence 1·5 inch
	Receiver taken off.			
	3 30 —	46½	58	idem
	3 37 receiver replaced and acid introduced			
	4 37 —	32½	59½	idem
	6 0 —	29½	59½	idem
	6 30 acid removed and potash substituted			
	8 0 —	36	59	idem

June

	Time.	Hygrom.	Therm.	Electrom.	
June 4th.	9 30 A.M.	32	56	divergence	1·25 inch
June 5th.	3 30 P.M.	30	53		1·1
	8 —	29½	54	idem	
	10 —	24	80	divergence	1·5
	Potash removed.				
June 6th.	11 0 A.M.	35	53	idem	
June 7th.	0 0 —	a moistened card placed in receiver.			
June 8th.	11 30 A.M.	40	50	divergence	1·25 inch
	2 0 —	40	61		1·5
	3 30 —	37½	77	struck	1 per minute
	3 45 —	37½	80		3
	4 45 —	39	76		2·5
	5 45 —	40	71		1·5
June 9th.	9 another moistened card placed in receiver.				
June 10th.	6 0 P.M.	42	60	divergence	1·25 inch
	6 40 —	40	69		1·5
	6 50 —	39½	73	struck	2 per minute
	7 25 —	39	77		3
	8 0 —	39¼	80		4·5

There are several circumstances deserving observation in these experiments, but one in particular; viz. that on the 5th of June, when the air had been dried by a long continued action of the alkali, the power of the column was not increased by a rise of temperature in its usual degree. Possibly the disks of paper had been also deprived of a part of the moisture which appears necessary to the action of the column.

XC. *Description of an efficacious Temporary Rudder. By Captain* John Peat, *of Bloomsbury Square*.*

Sir,— I beg to submit, through you, for the consideration of the Society for the Encouragement of Arts, Manufactures, and Commerce, a sketch of a temporary rudder, invented and used by me on board of the ship Cornwall under my command, on my voyage from this country to Jamaica, in January 1811; which ship was engaged by the Transport Board for the conveyance of 14 officers, 200 privates, 7 women, and 4 children, to Barbadoes.

In lat. 44° 0′ long. 19° 30′ on my passage out, I encountered a very severe gale of wind with a heavy sea, which carried away my rudder, and the rudder braces on the stern-post: I was, therefore,

* From *Transactions of the Society for the Encouragement of Arts, &c.* for 1813. — The gold medal of the Society was voted to Capt. Peat for this useful invention.

therefore, under the necessity of fixing a temporary one from the best materials I had then on board. On reference to a plan of Pakenham's temporary rudder, I found it impracticable to fix a rudder constructed on his plan, on account of the heavy sea to which we were exposed. It was therefore absolutely necessary, for the preservation of the lives intrusted to my care, that I should set about the construction of a rudder, which could be brought to act in a heavy sea, or under any circumstances whatever. This, I am happy to say, I accomplished, after a progressive improvement of fifteen days, and found this machine, when substantially fixed, to act in every point with the same effect as the regular rudder. One of the great advantages of the rudder invented by me is, that it can be shipped and unshipped at pleasure with the greatest facility, and under any circumstances.

At my request, a survey was held upon this rudder by the principal officers of His Majesty's yard, and all the old masters of the ships lying at Barbadoes; who were unanimous in their opinion that the same was a better rudder than could be procured at that island; and recommended my proceeding with the ship in that state to Jamaica, which I had no hesitation in doing.

I have had the honour to submit a sketch of this temporary rudder to the Honourable the Elder Brethren of the Trinity House, who were pleased to speak in high terms of the invention, and have subscribed for twenty copies of the above-mentioned engraving for their use.

I am, sir,
Your most obedient humble servant,
JOHN PEAT.

No. 11, Bloomsbury-square,
Oct. 24, 1812.

To C. Taylor, M.D. Sec.

SIR,—BEING requested by Captain Peat to confirm and state the particulars of what I know respecting his invention of a temporary rudder, that he had made on board the ship Cornwall, then commanded by him, when on a voyage from England to Jamaica:

I lament that I am not in London, where I have some papers whereby I could give dates of particular occurrences, which I stated fully in the Barbadoes newspapers on our arrival there. However, I recollect that it was on the night of the 3d of January 1811 that the ship parted with her rudder, when it blew a perfect hurricane, and which continued, without any abatement, the succeeding day and night. When the storm subsided,

Captain

Captain Peat devised a plan of making a rudder, with a spare fore-top-sail yard fixed over the stern, and by means of blocks reefed on each side, it served as a kind of paddle. The first trial was not a successful one, for want of a sufficient weight to keep the paddle under water; but which was soon remedied, for I think that it was on the 10th of January the ship answered the helm, and we proceeded on our voyage, every day discovering some new improvements; inasmuch that on or about the 16th he fixed his helm to the wheel, and we proceeded the rest of the voyage without any interruption, unless it was by negligence of the man who steered.

So confident was I of the safety and utility of this new invention, that on my arrival at Barbadoes I proceeded in her to Jamaica, when I might have gone in other conveyances, a distance of 1000 miles.

At Barbadoes, the master-attendant of the king's yard proceeded to sea in the Cornwall, by directions of Admiral Laforey, for the purpose of forming an opinion of the new rudder. On his return to Carlisle Bay, he declared that he would have no hesitation to sail to the Pacific Ocean with the rudder. He took a drawing of it, which the Admiral was to send to the Admiralty Board.

This new discovery I cannot too much praise, as being the means of once preserving my life; and the fatigue and labour which Captain Peat endured in accomplishing this machine, had nearly cost him his, by a severe fit of illness brought on by anxiety and exertions. If Captain Peat can derive any benefit from the British Government for his discovery, and of which I have bore testimony, I will add to his merits, by stating his humanity in affording comforts to 220 recruits whom he had on board, together with their wives, who were in the most deplorable situation occasioned by the storm, when the ship was unmanageable, and it being necessary to throw their beds overboard: many were sick, and received nourishment from Captain Peat's liberality.

I have the honour to be, sir,

Your obedient humble servant,

Birmingham, Nov. 10, 1812. JOHN RICHARDS.

To C. Taylor, M.D. Sec.

SIR,—IT having been represented to us by Captain John Peat, late commander of the ship Cornwall of London, in our employ, then engaged in the Jamaica trade, that he had submitted, for the consideration of the Society for the Encouragement of Arts, Manufactures, &c. &c. &c. an engraving and model of a temporary rudder, invented and used by him on his passage from

 this

this country to Barbadoes in January 1811, to which place he was conveying 235 of His Majesty's troops:

We consider that it would be doing Captain Peat a great deal of injustice, were we not to give him every credit due for so valuable and simple an invention, the efficacy of which has been proved by the distance run in so short a time, and the documents we have in our possession, with the information received from many experienced nautical men of great respectability, who were aboard at the time, and had an opportunity of witnessing the great ease with which the vessel was steered on the different points of sailing under all sail, and from the high terms with which it has been spoken of in this country by nautical men of the greatest experience and respectability, it cannot fail of being of great utility to the public; and we consider that great praise is due to Captain Peat for his perseverance and daily improvement in substantially fitting the machine, from the idea which first suggested the construction of it.

We have the honour to inclose a letter received from Mr. Lockwood, of His Majesty's Naval Yard, Barbadoes, accompanied with a sketch from that gentleman on the subject.

We have the honour to be, sir,

Your obedient humble servants,

THOMSON, OSBOURNE, AND CO.

Billiter-square, Nov. 12, 1812.

To C. Taylor, M.D. Sec.

GENTLEMEN,—I HAVE the honour to forward a sketch of the temporary rudder, by which Captain Peat governed the Cornwall to this island.

The apparent ease with which the ship reached this anchorage, the direct course she made under a press of sail, even studding-sails, and the account of its action, led me to investigate the circumstance minutely, and enable me to speak very confidently of its properties; and, in order that you may have yet more information than I have time to write, I inclose the sketch with a Barbadoes paper. It was my first intention to send the plan to Mr. Robert Blachford, Chartseller, Minories, for immediate impression, and to propose giving him a right or title to the plan, by sending me 200 copies. Captain Peat suggested the idea of my sending it to you.

I therefore, gentlemen, beg your acceptance of my humble labour; my sole wish was to render it public for the good of society, as in my opinion it not only supersedes Pakenham's rudder, which stands in such high repute, but every attempt of that nature hitherto made, and reflects the highest credit on Captain

Captain Peat for his progressive improvement upon the rude idea that first presented itself.

I have the honour to be, gentlemen,

Your very obedient humble seruant,

A. T. Lockwood,

Late Master R. N.

Master-Attendant of Barbadoes Naval Yard.

Naval Yard, Barbadoes,
Feb. 18, 1811.

To Messrs. Thomson and Co.

Opinions of Officers in His Majesty's Royal Navy, respecting Captain Peat's *Temporary Rudder.*

Captain Losack, of H. M. R. N., thinks that the simplicity of Captain Peat's temporary rudder is its great qualification, and never saw any thing so good under every circumstance; that it can be shipped in cases where Captain Pakenham's cannot; that few merchant-ships have spare caps; that every merchant-ship has the materials to form Captain Peat's rudder; that Captain Peat's method is much superior to that which was adopted by Mr. Nicholson, described in his "Treatise on Practical Seamanship," and as used by him for the Grafton and Elizabeth.

It appears to Captain Losack, that with Captain Peat's rudder a ship will tack, but not with the rudder described by Mr. Nicholson; which is a matter of great consequence.

Captain Hanwell, of H. M. R. N., is of opinion, that Captain Peat's invention can be executed by any merchant-ship, and readily shipped in any weather; that Captain Pakenham's rudder cannot be so shipped; that he has no doubt of a ship tacking with Captain Peat's temporary rudder. He adds, that he agrees generally with Captain Losack in the observations made by him.

Captain Jackson, of H. M. R. N., thinks Captain Peat's temporary rudder has much superiority over every other invention for the purpose that he is acquainted with; that it possesses great simplicity, and that ships of all descriptions have the means of constructing it.

That in cases of heavy sea and bad weather, he is of opinion that it may be more easily applied than Captain Pakenham's rudder, but he thinks Captain Pakenham's rudder superior when once applied.

That Captain Peat's rudder appears to him more generally applicable to merchant-vessels and small ships of war than to ships of the line; that he cannot judge with precision of its power, from not having seen it in practice, but he believes it can tack in all common cases.

 Re-

Reference to the Engraving of Captain PEAT'S *Temporary Rudder, which can be quickly fitted out by a Ship's Company when at Sea, and from such Materials as they always have on board.* See Plate VI. fig. 1, 2, and 3.

Plate VI. fig. 3. shows all the part of this contrivance detached, and ready for launching over the ship's stern; fig. 1. shows an elevation of it in action; and fig. 2. a plan answering thereto. The invention consists of a top-mast, placed out of the ship's stern, having planks fixed at the end of it, which go edgeways through the water; the mast is attached to the stern by a kind of joint or socket, on which, by proper guys, it can be inclined in an angle, with the keel either larboard or starboard; and will steer the ship as effectually as a proper rudder.

To fit out a ship on this plan, the following materials must be collected and prepared as soon as the rudder is gone, or expected to be lost.

1st. A spare top-mast AB, fig. 2. and two top blocks *hh*, which must be securely lashed on at each end of it.

2d. A top-sail-yard CD, which must have cleats, nailed securely upon it, forming a shoulder at the end C, which is to be kept on board; and at the opposite end must be framed a case *eee*, formed of pieces of oak plank, nailed firm to the yard, on each side, the left or foremast part being chamfered off and leaded: so that by presenting a narrow edge it may make the less resistance in passing through the water.

As the taff-rail will not, in general, be found sufficiently strong to resist the force of the machine; a frame *klmn*, must be composed of substantial pieces of oak, and erected behind it, upon the deck; it consists of two uprights *mm*, set up from a sill *n*, which is spiked down upon the deck: and they support a rail *p*, which is firmly lashed to the taff-rail: and the whole is strengthened by three shores *kk*, stepped from the deck to the uprights *mm*: between these is supported a piece of oak *l*, $4\frac{1}{2}$ inches thick, with a hole through it, of a proper size to receive the end *f*, of the yard: the hole is leathered inside, in which the end *f* of the yard acts with perfect ease, and can swivel about in all directions; the tapered end *f* of the yard is leathered also, that the whole may act easily.

While these parts are preparing by one part of the crew, some should get the top-mast A athwart-ship, over her quarter, and make it fast by proper lashings; the others should be ready with the several tackles which rig it out, to give motion to the rudder when in the water. They are as follows: two principal guys, *aa*, made of rope which is $5\frac{1}{2}$ inches girt, and should be of very good materials; they are to be made fast to the boards

at

at *ee*, and carried through the top blocks at *hh*; the end has the block *r*, of a liff tackle purchase, turned in; the other block being lashed to one of the stanchions on the ship's quarter, and the fall carried through the leading blocks *s s* to the steering-wheel F, situated upon the ship's deck.

A topping-lift, *b b*, suspends the weight of the rudder from the spanker-boom G: it is a gun tackle purchase, and is of great use in getting the machine out of the water, and also to assist in getting it in; the fall of the purchase is carried along the boom to a cleat spiked to it.

c is another topping-lift from the rudder to the mizen-mast head; it likewise assists to get in the machine, as well as for a substitute, should either of the guys break; it should be $3\frac{1}{2}$ inch rope.

Two short luff tackles *dd*, fixed to the taff-rail and cross piece *p*: they are for the purpose of holding the end of the mast firm in the socket, and materially aid in placing and displacing it.

A jack-stay, *i*. This is a rope stretched tight from the planks at *ee*, to the other end of the mast; on this a weight, *t*, is suspended. The machine is ballasted sufficiently for common occasions when the ship is going less than eight knots; but this additional weight, consisting of shot sewed up in canvass, is to assist in keeping the whole of the machine in the water, when going more than eight knots; and when going less, to be hauled up by the line *v*.

The mode of operation in this machine will appear evident from the above description: the steering wheel F, being turned either way, acts to bring in the fall of one of the guys *a*, and give out the other, producing an inclination of the rudder, as in fig. 2, so as to put the ship about as expeditiously as a common rudder; and upon the same principle, viz. that of making a resistance to the ship's motion, on either side of her keel, at pleasure. The ship Cornwall, in which the contrivance was first tried, had her wheel fitted close to the rudder-head, and she steered with a short tiller abaft the rudder. In ships fitted in the common manner, the tackles would lead to their wheels in a similar way; the only alteration from the figure being, that the wheel is placed much further forward on the deck, and the leading blocks *ss*, are of course placed opposite to it.

Certificate and further Observations from Mr. A. LOCKWOOD, *Master-Attendant of H. M. Naval Arsenal, Barbadoes.*

The Cornwall lost her rudder on the morning of the 4th of January, and got this machine over on the 7th at noon; arrived at Barbadoes on the 11th of February, and at Port Royal on the 23d. During the passage from Barbadoes to Jamaica, this machine

chine acted with great effect, the ship carrying royals and all studding-sails night and day, and for several hours together going 10 and $10\frac{1}{2}$ knots; during which time she required less wheel than with the common rudder, and at no time did the temporary rudder require to make an angle of more than 10 degrees, either in the act of wearing or tacking.

This simple and truly ingenious method of governing a vessel in distress, I recommend earnestly to the notice of all persons subject to casualties, that may require an expedient of this kind; and although temporary rudders are no new subject, yet the one here delineated is unquestionably the best ever held to public view. Little more need be said in its praise, than the concurrent opinions of seven professional men (whom I know to possess clear judgement) that the rudder was superior to any that could be made at this island, and they had no hesitation in recommending Captain John Peat to proceed on his voyage to Jamaica, without any alteration in it whatever; and I can venture to assert, without hesitation, that the machine in question not only supersedes Pakenham's rudder, which stands in such high repute, but every attempt of that nature hitherto made, and reflects the highest credit on Captain Peat for his progressive improvement upon the rude idea which first presented itself.

The circumstances favourable to the machine are, first, its being composed of such materials as vessels of any description are possessed of; secondly, the simplicity of its composition superseding the absolute necessity of a carpenter; thirdly, that it may be constructed and put in action, even in a gale of wind, in two, or at most three hours. Its properties are, 1st, that it can be shipped at pleasure without delay, and with very little trouble; 2d, that it cannot, by any shock or violence, be rendered unfit or useless; 3d, that the guy tackle fall, being brought to the wheel, requires only the same force to steer as the common rudder; that the ship is under complete command, as will appear by the ship's log-book, in all the following cases:

"Gale of wind, heavy sea, wind quarterly."

"Light winds, heavy swell."

"Fresh wind, spanker, main-sail and all sail, on a wind, off the wind."

"Staying and wearing."

The sole object of this, my humble labour, being to promote, in however small a degree, the good of the nautical world; I cannot in justice drop the subject without suggesting what, in my opinion, would be, with little additional trouble, a very great improvement. The body of dead water occasioned by the flat open part x, of the planks at the end of the yard, tends to impede the ship's progress, and to force the case upwards, which consequently

consequently strains the guys when going very quick. The dotted lines at x, fig. 3, I propose to be a piece of plank to be continued on the end of the topsail-yard, so formed or filled up as to carry off the body of water complained of.

(Signed) ANTHONY LOCKWOOD.

XCI. *Report of the Progress of the Sciences in France in* 1813. *By* J. C. DELAMETHERIE*.

ANIMAL PHYSIOLOGY.

On the influence which the temperature of the air exercises in the chemical phænomena of respiration.

RESPIRATION has been latterly regarded as a kind of combustion, viz. that of the carbon and hydrogen contained in the venous blood. The oxygen absorbed by this combustion forms carbonic acid and water.

It has been endeavoured to ascertain the quantity of atmospheric air which a man of a middling stature inspires at each inspiration, and expires at each expiration. It has been supposed that it was from 20 to 30 and even 40 cubic inches; but I have shown that this supposition is not correct. A man of a middling stature inspires only a few inches of atmospheric air. Now the atmospheric air contains only about one fifth oxygen, or 0·21.

In the act of respiration a very small portion of this oxide inspired is combined with carbon, and forms carbonic acid.

Another portion of this oxygen is combined with a portion of inflammable gas, and there is a production or disengagement of water. But the greater part of this light portion of oxygen is not combined, and it is found in the air expired mixed with carbonic acid.

But Delaroche, like myself, has discovered that there is a *production of azote*.

He made a great number of experiments in order to determine the influence which the temperature of the air exercises in the chemical phænomenon of respiration. He placed at different temperatures animals in *manometres* or glass vessels with large apertures, hermetically closed by copper plates and screws. If we compare, he says, the results of the experiments made on one and the same animal placed in the same circumstances and at *different temperatures*, we shall see that almost in all the experiments upon the cold-blooded animals, the quantity of oxygen absorbed was a little greater when the temperature was low than when it was high.

* Abridged from the *Journal de Physique, de Chimie, et d'Histoire Naturelle*, for Jan. 1814.

The

The difference between the quantities of carbonic acid formed at different temperatures is still less considerable.

In all cases there is less carbonic acid produced than oxygen absorbed. *I concluded*, he says, *with M. Berthollet, that there was a production of azote.*

In an experiment made upon a hare, the manometre contained 0·7900 azote, 0 2100 oxygen.

After the experiment, the azote was 0·7991, the oxygen 0·1516, and the carbonic acid 0·0416.

There was therefore a production of 0·0091 of azote, and 0·054 of oxygen absorbed. I had remarked the same phænomena, and I have observed in my Essay upon pure Air, that in the air expired there was always a production of a portion of *impure air*, the azote of the new nomenclature.

Spallanzani has proved that a contrary effect takes place in the cold-blooded animals. My experiments, says M. Delaroche, prove also that heat augments in a most remarkable manner the activity of respiration in these animals. The quantity of oxygen absorbed by frogs exposed to a heat of 27° has been in one experiment *double*, and in the other *quadruple*, to what it was when the external temperature did not exceed six or seven degrees.

ANIMAL HEAT.

Respiration being regarded as a kind of combustion, it has been considered as the principal cause of the heat of animals; but I have shown that too much stress has been laid on this cause.

1. *a.* We find that a man of middling stature only takes in at every inspiration a few cubic inches of atmospheric air. Now atmospheric air contains but little more than one-fifth of oxygen, or 0·21.

b. There is but a small quantity of this oxygen combined in respiration, certainly less than a cubic inch.

2. *a.* A man who sleeps tranquilly takes cold, although he breathes quite at his ease.

b. If he takes exercise, he acquires heat, and even perspires.

c. An animal exposed to a severe cold may perish if it does not take exercise. If, on the contrary, it moves or carries burdens, it preserves its life.

d. Consequently the muscular motion has the greatest influence on animal heat.

3. Oxygen gas contains very little heat; therefore the small portion which is combined in the act of respiration has produced very little heat.

I have concluded from these facts, that animal heat proceeded

ceeded but in a very trifling degree from the caloric extricated from the oxygen inspired.

4. If animal heat proceeded from respiration, or from the combustion of the carbon in the act of respiration, the lungs ought to possess a greater degree of heat than the other parts of the system, as Brodie has asserted, which is not the fact. He thinks that animal heat is in a great measure under the influence of the nervous system and of the brain.

In the muscular movements the nervous system is in a state of activity more or less considerable; this is the reason that heat is produced in the animal body.

5. The fermentation of the various animal liquors contributes much to the heat of animals, for we know that every fermentable substance contracts heat. Now all the animal liquors are in a perpetual state of fermentation.

6. There are continual combinations in the animal œconomy which give new products, such as the phosphoric, uric, sebic, acids, glutine, fibrine, &c. &c. Now all these combinations are uniformly accompanied with an extrication of caloric.

7. The galvanic action is powerfully exerted among the various heterogeneous particles of the bodies of animals which ferment. This galvanism is very intense in the electric eel, &c. and contributes powerfully to animal heat.

BOTANY.

Picot la Peyrouse has given a complete history of the plants of the Pyrenees. Cassini has published an elegant work on the Synantheræ, or Syngenesiæ of Linnæus, the Composita of Tournefort.

Palisot de Beauvois, having ascertained that the methods for the study of the Graminea were imperfect, has proposed a new one, which he calls *Agrostographia*. Desvaux has published some additional observations on the Lycopodiaceæ, of which Jussieu and Mirbel have furnished us with an extract.

Bonpland has published the second number of the rare plants of Navarre and Malmaison.

Redouté continues his work on the Liliaceæ. Mirbel has published the description of various new fruits, and also a History of Botany from its infancy among the Greeks to the present time.

Decandolle has given an elementary treatise on botany, or an explanation of the principles of natural classification, and of the art of describing and studying vegetables. Tristan has published a fine work on the budding of plants.

VEGETABLE PHYSIOLOGY.

Under this head M. Delametherie, after stating it to be his opinion

opinion that the analogy between the organization of plants and animals becomes daily more evident, gives a sketch of the memoir of M. Link on the structure of vegetables, which has been already given at length in the Philosophical Magazine.

MINERALOGY.

Cordier has made some researches respecting the stone called by some mineralogists *water sapphire*, and by some others blue quartz. It is brought from India, and particularly from Ceylon. Its gravity is 2·580, which removes it from the sapphire. Its colour is blue at first sight; but when we view it in a plane perpendicular to the direction which had shown the blue, its colour is of a clear brown inclining to gray. This double colour made M. Cordier suppose that this stone was a variety of the dichroïte.

Of the Ligurite.—This stone has been discovered by Viviani in the mountains of Liguria near Genoa. This substance, which I have not seen, appears to several naturalists to be a variety of Titanite.

Of the Chlorite in secondary strata.—It was once supposed that chlorite existed only in primitive strata, but it has since been found in secondary also. It is met with in the vicinity of Paris. Risso has also observed it in the environs of Nice. Near the castle of Nice, he says, there are regular beds of *chloritous* marl with belemnites. These facts show that minerals, which have been thought peculiar to primitive strata, may be transported into secondary. Lambotin has found fluor also in the environs of Paris; but it is crystallized with calcareous spar, which proves that it has been brought there and deposited.

Of the Lherzolite.—I have given this name to a stone from Lhers in the Pyrenees, where Le Lievre found it: it appears to be a variety of smaragdite.

Charpentier junior saw large beds of it in the same place. He regards it as a variety of augite, but augite has not as yet been found *en masse*. Vogel analysed it, and found as follows:

Silex	45
Alumine	1
Lime	19·50
Magnesia	16
Oxide of iron	12
Oxide of chrome	0·50
Oxide of manganese	*a slight trace.*
Loss	6

This analysis approaches very closely to that of the green smaragdite, made by Vauquelin.

Picotite.—We sometimes find in the heart of the masses of lherzolite,

lherzolite, a blackish substance which seems to have some connection with Gadolinite. Charpentier gave it the name of Picotite, from the name of Picot de la Peyrouse.

Mines of Tin discovered in France —Tin mines have been discovered near Limoges, and others at Pirac near Nantes.

Of Rocks.—The year 1813 has supplied us with various works upon rocks; but they have not all met with the approbation of mineralogists, and they continue to adopt the nomenclature of rocks given by the Germans, imperfect as it is.

Pinkerton has published a work of considerable extent upon rocks.

Other mineralogists have also proposed a new nomenclature for rocks : but it may be fairly thought that they wished them to have been unanimously rejected, for they are removed from all the *philosophical principles of language*. They gave, for example, the name of *mimosa* to a rock composed of feldspar and augite. Now there is no naturalist, or even amateur, who does not know that a celebrated plant, viz. the sensitive plant, is called *mimosa* ; when we read the word mimosa, who can tell whether a stone or a plant is alluded to ?

It would be useless to detail the other defects of works so unanimously disapproved.

Crystallography.—After noticing Dr. Wollaston's labours on the molecules of crystals, M. Delametherie proceeds :—Werner told me during his last visit to Paris, that he thought the figure of the primitive molecules of matter was spherical.

Preschtal adopted the same opinion. Bodies, he says, do not crystallize but when they are in a liquid state. Now every fact seems to prove that the molecules of liquids are spherical. Descartes had published the same opinion. He said that the molecules of his two first elements, fire and the luminous fluid, were spherical.

GEOLOGY.

This branch of science has been this year the subject of the labours of a great number of scientific men. Geology is equally advanced with the other branches of natural philosophy. It has problems, it is true, which have not been as yet resolved, but there are problems in all the other natural sciences.

Of primitive Earths.—The primitive earths form the major part of our globe. The geologist cannot therefore study too minutely those with which we are acquainted, such as granites, porphyries, gneiss, schists, amygdaloids, the metals, the anthracites, &c.

Charpentier jun. has given some interesting details respecting the Pyrenees. Hoff and Jacobi have visited, as intelligent mineralogists,

neralogists, the Thuringewald, and Braun Neergard has given us an account of their journey. In similar researches we ought chiefly to mark out the chains of primitive earths which traverse the surface of the globe. I have distinguished for instance five great masses of primitive earths in France:

1. A portion of the Alps.
2. The Cevennes.
3. A portion of the Pyrenees.
4. The mountains of Bretagne.
5. The Vosges.

Secondary Earths.—On this subject Risso has given a description of the calcareous earths in the vicinity of Nice. All these secondary earths contain quantities more or less considerable, of fossils, *i. e.* traces of animals and vegetables.

The knowledge of fossils may afford some notions as to the formation of the strata of the globe in latter times: this branch of geology, therefore, is making rapid progress, as we are better acquainted with living animals and vegetables. I have distinguished three fossil orders:

Marine fossils deposited at the bottom of the sea.

Fresh-water fossils deposited in fresh water.

Land fossils deposited in the bowels of the earth, or buried by the fall of mountains, without having been touched by water.

We can also distinguish

The fossils of organized beings which live in the ocean.

The fossils of organized beings which live in fresh water.

The fossils of organized beings which live on the continents.

FRESH-WATER STRATIFICATIONS.

It cannot be doubted that there were strata formed in fresh-water lakes after the lowering of the level of the water of the ocean, as I have proved in my Theory of the Earth, tome v. p. 137.

Lamanon long since recognised fresh-water shells in the strata of the environs of Paris; which made him say that several of these strata, and particularly the chalks, were formed in *fresh-water lakes*. He has said the same of the chalks of Aix in Provence.

Coupé has also given accurate descriptions of the soils in the environs of Paris: he there recognised fresh-water shells.

Cuvier and Brongniart have adopted the opinion of Lamanon with respect to certain soils in the environs of Paris. They suppose that they have been formed in fresh water, because fresh-water shells are there found. They found at Montmartre a *cyclostome of a blackish colour*.

Brongniart, Prevost, and Desmaret jun. also observed fresh-water shells in part of Auvergne. Bendaut has also found *lymneæ* (fresh-water shells) near Vaucluse. They have also been found

found near Valence in Dauphiny; near Roanne, and Orleans; in the plains of Ulm, and at Rome; in Silesia, and at Burgos in Spain.

The Isle of Sheppy at the mouth of the Thames has also presented some fresh-water shells. Faujas has also seen them on the banks of the Rhine, near Mentz, and at Frankfort on the Maine.

Risso has made us acquainted with the shells which are found in the environs of Nice.

"The waves," he observes, "acting continually on the rock, detach these marine petrifactions, polish them, mix them with the sea shells of the present day, and the remains of the terrestrial molluscæ carried along by the pluvial waters:—the whole form new deposits, which will perhaps be ænigmatical subjects of meditation for future generations.

Most of these naturalists have said that the soils in which these fresh-water shells are found had always been formed in these fresh waters, and they call them *soils of fresh-water formation.*

It cannot be denied that there have been strata formed in fresh water after the retreat of the sea. We see them formed every day in lakes of fresh water; but we cannot say that all the soils in which fossil fresh-water shells are found have always been formed in fresh water. For we have seen in these very soils, as at Montmartre, shells which are not sea shells, bones of quadrupeds of *terra firma*, &c.; the latter evidently washed by currents of water. The fresh-water shells must also have been brought there by the waters, as observed by Risso at Nice. In fact, it cannot be doubted that the fresh water which daily flows into the sea, carries with it the remains of animals and vegetables of terra firma, and the remains of animals and vegetables which live in fresh water. Thus the waters of the Seine may carry to Havre planorbes, lymneæ, bulimi, from the fresh water of the environs of Paris.

The greatest part of these shells is broken, as observed at Grignon, but some are preserved perfectly entire.

FOSSIL QUADRUPEDS.

We have had a great number of observations on the fossils of the mammiferous and oviparous quadrupeds. Cuvier has brought them all under view in one work, and added his own observations.

He has spoken of 78 fossil quadrupeds, as well viviparous as oviparous.

Twelve are analogous, he says, to living animals: these are,

1. A kind of hippopotamus.
2. The stag.

3. The goat.
4. The ox.
5. The aurochs.
6. The musk ox.
7. The deer.
8. The hyæna.
9. The wolf.
10. The dog.
11. The horse.
12. The sheep.

Sixteen or eighteen other species of fossils appear to be analogous to existing *genera*, but not to existing species: these are,

The elephant.
The rhinoceros.
The tapir.
The small hippopotamus.
The bear.
The jacuar.
The hare.
The fox.
The crocodile.
The turtle, &c. &c.

Finally, 48 other fossil species do not appear analogous either to existing species or existing genera: these are,

The megalonix.
The megatherium.
Five species of mastodonta.
Ten species of paleotherium.
Five species of anoplotherium.
A petrodactyl.

Some authors have spoken of fossil remains of apes. Swedenborg says that the bones of the sapajon have been found in copper-mines at Menungen; but Cuvier thinks that these bones rather belonged to the bat genus.

Fossil bones of the human species have also been spoken of. Cuvier thinks that these bones are not human bones.

Fossil Birds.—There are fossil birds. I have stated in my Theory of the Earth, that I have seen them in the chalks of Montmartre. Doubts have been raised improperly on this subject, for soon afterwards I saw several. But we do not know of any others.

Fossil Fishes.—In the great number of fossil fishes which have been observed in several places, as at Mount Bolca, several have been recognised similar to those of the present day.

Fossil Shells.—Fossil shells have been observed from the highest antiquity. The priests of Egypt spoke of them to Herodotus.

tus. Pythagoras had seen them, according to Ovid. But our knowledge in this respect has made great progress latterly.

Lamarck and Defrance have made us well acquainted with those in the environs of Paris. Cuvier, Brognard, Faujas, and Brard, have laboured in the same field with success.

We distinguish three orders of fossil shells, viz. sea shells, fluviatile, and terrestrial.

The sea shells are the most numerous. In the depôt of Grignon alone, nearly 600 species are found, the species or genera of which exist at present in different seas, far asunder.

Most of the same species are to be found at Courtagnon near Rheims, according to Lamarck.

Fluviatile and terrestrial shells are less numerous. Daudebert Ferrusac counts 83 known species: 21 helices and bulimi; 1 vertigo; 24 lymneæ; 10 planorbi; 1 physis; 5 cyclostomes; 11 paludines; 1 potanidis; 3 melanopsides; 3 melanies; 2 shells approaching to the bulimus glans; 1 neritine. Twenty-five of these species, he adds, have their analogous species in the same soil. Eight have their analogous species now existing in the Indies and America. Fifty have no analogies, so far as has been hitherto discovered.

Of Crustaceous and Fossil Insects.—At Maestricht as well as at other places crustaceous fossil insects have been discovered, and every naturalist is acquainted with the insects in amber.

Of Fossil Madrepores.—All these productions in the fossil state are very abundant.

Of Fossil Vegetables.—Fossil vegetables are extremely abundant, since they enter into the composition of the immense quantity of coal and peat moss with which the earth abounds.

We also find great quantities of fossil and petrified wood, &c. Some of these productions seem never to have been removed from the place where they are found, such as the forest of Palm-trees observed by Audenrieth on the banks of the Neekar, and the forest observed by La Fruglaie on the sea-coast near Morlaix.

But the greater number appear to have been carried to distances more or less considerable.

a. Generally we only find separate portions of the skeletons of animals, such as the teeth, thigh bones, and never the entire organized beings.

b. We find collected in the same depôt, marine, fluvial, and terrestrial fossils.

c. Fossils have their analogous fossils in countries far distant from each other.

The above facts therefore indicate that these fossils have been carried

d. by the currents which take place at sea; *e.* by those which take place in lakes; *f.* by the currents of rivers; *g.* by catastrophes which have happened on the surface of the globe.

CATASTROPHES ON THE SURFACE OF THE GLOBE.

Geologists, proceeding upon the various facts exhibited by the theory of the earth, conclude, with the priests of Memphis and all subsequent philosophers, that various catastrophes, more or less considerable, have happened on the earth's surface. Some even suppose that there have been general catastrophes.

In my Theory of the Earth I have cited the following causes as likely to have produced these particular catastrophes: viz.

1. *Particular Inundations.*—*a.* Abundant rains which have swoln the streams of the Nile, the Niger, and the Menau. *b.* Overflowings of lakes, of which history makes mention in abundance: such were the deluges of Ogyges, Deucalion, Prometheus, &c. *c.* Violent winds have produced great inundations in Holland, by swelling the sea, as in 1218 and 1646.

These sea waters have remained a more or less considerable time on peat mosses and other strata formed in fresh water, and deposited fossil shells. Poiret observed near Soissons peat mosses containing fluvial shells, covered with strata containing sea shells, cerites, venuses, and oysters.

d. Explosions of subterraneous fires throw up part of the sea, and cause particular inundations.

e. The fall of some mountains has produced some local inundations.

2. Earthquakes have occasioned many singular catastrophes on the surface of the globe; but these have been confined to certain countries only. That of Lisbon in 1755 shook some countries very distant; but its effects were far from producing a general catastrophe.

The dreadful explosions of the enormous volcanoes of Mexico and Peru produce only limited catastrophes.

3. The passage of a comet close to the earth has also been regarded as a cause which must have produced a great catastrophe on the surface of the globe, by swelling the waters of the ocean; but all astronomers are now agreed that this hypothesis has no probable foundation.

4. But there is another cause which ought to produce, after some centuries, great changes on our globe: this is the increase of its mass, which I have proved ought to take place. I have said, in my Theory of the Earth, that "a great part of the secondary strata is formed of the remains of organized beings: such are the bitumens which form immense and very deep strata,

fossil

fossil plants, shells which form the major part of a great number of stones, fossil bones, several saline substances of these stratifications."

The mass of the terrestrial globe augmenting, ought to produce changes in its relations with the other planetary bodies: their mutual attractions will change. The sun, on the other hand, perhaps loses part of its mass. It appears clear to me, therefore, that after some centuries great changes will take place upon our globe. This was the doctrine of the ancient philosophers. Ovid makes Pythagoras say in his Metamorphoses, book xv.

> Nihil equidem durare diu sub imagine eadem
> Crediderim.

Lucretius has also said, book v.

> ———————— multosque per annos
> Sustentata ruet moles et machina mundi.

But of what nature will these changes be? We are not in possession of a sufficient number of facts to be able to predict their nature.

As to the hypothesis of De Luc, who has advanced that the *existence of the human species is posterior to that of other species*, it appears to me to be defective. We do not find, he says, fossil human skeletons. To this I answer:

1. We find only about 12 *species* of known quadrupeds, and from 16 to 18 *genera*. Are we to conclude that none of the other species and genera existed at the epoch in question?

But, it is added, the human species is now-a-days so multiplied.—It is easy to answer that the human species was not, at that period, so numerous as it has been since large societies have been formed.

3. Lastly, we know that the bones of the largest animals which die in the fields, in the woods, &c. are speedily decomposed if they are exposed to the air. We do not find in our forests any bones of our modern boars, stags, and wolves, nor in Africa any bones of the elephant or rhinoceros.

4. Those which are fossil have been preserved, therefore, by being enveloped soon after the death of the animal in earth or sand. This event must have taken place under extraordinary and unexpected circumstances. Thus we meet with very few fossils of quadrupeds, fishes, birds, and vegetables, in comparison with the immense quantity which has existed.

It is otherwise with fossil shells: they are very abundant. Nevertheless this quantity is very small when compared with those which have existed; and we ought to consider well, that we find in some places immense heaps of shells of various coun-

tries, with fossils of quadrupeds, whales, &c.: they seem to have been heaped together by local circumstances.

Fossil vegetables are also very abundant. All the species of vegetables and animals now in existence may not have commenced their existence at the same epoch and in the same countries: thus the animals of the continents have begun to exist long before the sea animals; but nothing shows that the human species began to exist after the ape and monkey species, &c.

Epochs at which Fossils have been deposited.—It is difficult to assign these epochs; but it may be asserted in general, that

a. The fossils which are found in calcareous, gypseous, schistous, or bituminous stones, were deposited when these stones were formed, and consequently are the most ancient of fossils.

b. Ravines (*breches*) having been formed subsequently to stones, the fossils of ravines are therefore posterior to those of stones.

c. Coal and peat moss are also posterior to secondary stones. The fossils which exist in coal and peat moss are therefore also posterior to those of stones.

d. Alluvial strata are also posterior to the above; consequently the fossils contained in these soils are in general more modern.

e. Caverns have never been discovered until after the sea had retired. The troglodyte animals could only have retired thither at very recent periods, and left their remains there. Fossils of sea animals have never been found there.

Volcanoes.—The city of Caraccas in South America has been destroyed by an earthquake, and various parts of Europe have experienced similar visitations; but they were not so violent, nor attended with any new phænomena. There has also been an eruption of volcanic substances from the sea near the Azores. Earthquakes may be occasioned by volcanic eruptions: the gases which are emitted from inflamed substances pass through the chinks of the strata with rapidity, and produce shocks more or less violent. But other earthquakes, such as those which take place without any appearance of volcanic eruptions, seem to be occasioned by the galvanic action of different heterogeneous particles of the globe, particularly metallic substances.

In order to measure the intensity of earthquakes, an instrument called the Elkysmometer has been invented. This instrument makes oscillations when it is shaken. We estimate the intensity of subterranean commotions by the size and number of the oscillations of the instrument.

GEOGRAPHY.

Gosselin, in his Inquiries into the Geography of the Ancients, has shown that the Greek geographers, Eratosthenes, Hipparchus,

thus, Posidonius, Strabo, and Ptolemy, had drawn their information from a people of still greater antiquity. He supposes them to have been the Hindoos. But it appears more probable to me that it was the Tartars and Chinese. The latter were acquainted long before any other nation, with printing, the mariners' compass, gunpowder, the use of silk, and astronomy.

The knowledge of the mariners' compass announces that they were great navigators, and consequently they must have had an extensive knowledge of geography. Their astronomical acquirements gave them the means of ascertaining longitudes and latitudes. The desire for travelling is now general, and learned travellers daily extend our geographical knowledge. Morier has given us some geographical details respecting Persia, Armenia, and Asia minor. Kimmel has published his Journey to Mount Caucasus. Mawe has made us better acquainted with the Brazils, and given us some interesting details on the mineralogy of those countries, their diamond and gold mines, &c.

[To be continued.]

XCII. *On the Assay of Minerals by means of the Blowpipe. By M.* Haussman, *Inspector General of Mines at Cassel**.

Circumstances necessary to be observed in the preparation for the experiment: volume of the fragment to be assayed.

It ought to be proportioned to the size of the flame to which it is exposed. If the aperture of the blowpipe be only the diameter of a pin, the volume of the fragment ought not to be larger than a pepper corn. In order to support the fragment, we may use: 1. Pincers of platina, or with platina points at least. 2. A small glass tube or cylinder, the end of which is to be softened in order to fix the fragment to it. 3. A small bit of cyanite, according to Saussure's method. 4. Charcoal of good quality, particularly poplar and elm, flattened on one side, and with a small hole in it in which to place the fragment. We may cover this piece of charcoal with another piece; and in this case a passage must be made for the flame, which ought to fall on the fragment contained in the hole. 5. We may compose a stud for support with charcoal dust pounded in mucilage of gum tragacanth. We must form of this paste parallelopipedons, and dry them slowly. 6. We may use a small spoon of gold or silver, but above all of platina, the end of which is fixed in a pipe or in a wooden handle, to preserve the fingers from being scorched.

* *Journal des Mines,* Jan. 1811, vol. xxix. p. 61.

 As

As to the method of presenting the fragment to the flame of the blowpipe, we may place it, 1. Half within the flame. 2. In the yellow part of the flame, which produces oxidation. 3. In the blue part, which effects the process of reduction.

Duration of the experiment.—It depends on the different parts of the flame where the fragment is exposed, and upon the degree of intensity of the heat. This intensity depends on the volume of air which the blowpipe gives out according to the breadth of its aperture, which we may vary occasionally if the blowpipe be properly constructed: the best is of metal, and in three pieces, a tube, a reservoir, and a point in the form of a cone; the reservoir, placed between the tube and the point, retains the humidity of the breath, and serves at the same time as a moderator, by means of the compressed air which it contains. We may adapt to this reservoir tubes of different diameters and points with different apertures. We may also procure a stronger or weaker current of air. The abundance of the current of air depends on the power of the blowing instrument: the air is furnished to the blowpipe either by means of a bellows with a double vent, or by means of the mouth: the air is taken in through the nostrils, and blown out through the mouth. This is the method most in use and most convenient, and renders the operator more master of the experiment. The stud or support of the fragment assayed is to be held in the left hand, and the blowpipe in the right, with the elbows resting on the table. We may also manage the experiment by placing the fragment on a fixed or moveable support; but it is better to hold it in the hand.

The intensity of the flame is different:

1. According to the state of the air which issues from the blowpipe, it is clear that the stream furnished by the bellows is better than that from the mouth: the difference has but little influence, however, on the result of the experiment.

2. This intensity is different according to the nature of the light employed: a candle, lamp, or taper. A candle is better than a lamp, but a wax taper is best of all. The wick as well as the taper ought to be flat, and the flame has of course a flat appearance. The current of air should be so directed as to make the flame act upon the fragment at an angle of 45 degrees.

3. Its intensity is different according to the different parts of the flame where the fragment is exposed. The place where this intensity is strongest is the extremity of the blue point of the flame. We get at this point by management, moving the fragment about till we find it reddish.

In the course of the experiment we ought to observe the various phænomena presented by the fragments under assay.

In those which exhibit no considerable change we remark the various

various tints which they assume on becoming red, from the faint red to the white.

Phosphorescence, when it takes place, presents also various colours, which change sometimes, as we see in the apatite and fluor spar. We may remark also the colour assumed by the flame itself; the sulphate of strontian, for instance, gives to the blue part of the flame a reddish tinge.

As to changes in the mineral, these may take place without any alteration in its form, such as the change or loss of colour. The surface may become deeper, either in whole or in part, as is the case with pyrites.

The yellow oxide of iron becomes totally red, the flowers of cobalt blue, &c. There is also a change of colour, or loss of lustre, in white mica, lamellated gypsum, &c. There is a change in the refrangent property, when the molecules of a fragment which was diaphanous separate, from the loss of the water of crystallization, or the enlargement of their interstices, which frequently gives an opportunity of discovering the texture of various substances, such as ponderous spar, &c. There is also a change of consistence: it increases, as in potter's clay, and diminishes in others, such as lime, which the action of heat renders porous. Various odours and tastes are also developed by heat; and there may be an alteration of form without change of substance, by the liquefaction of the water of crystallization, of alum, borax, &c.

Decrepitation.—When a mineral bursts into fragments, we must distinguish whether the noise be great, and the pieces large, or the contrary.

Evaporation.—In this case the molecules of a substance are detached in form of vapour, without any alteration in their substance; as water, mercury, &c.

Exfoliation.—Separation of the laminæ of a mineral, from which an increase of volume results; as in lamellated gypsum, apophyllite, stilbite, triphane, &c.

Efflorescence.—Is the formation of small mossy excrescences on the edges of the fragment, a phænomenon probably produced by some disengagement of gas.

Formation of air bubbles.—Solitary air bubbles are formed on the surface of the mineral, as in the pyrophysalite, probably by the disengagement of the fluoric gas.

General swelling.—When the mineral increases in volume by the formation of a multitude of small bubbles, which give it the appearance of a froth; is this to be ascribed to a disengagement of gas, or a development of vapours? It is difficult to say which. This fact is remarkable in the meionite, the lepidolite, &c.

Boiling.

Boiling.—Is when a mineral entering into fusion presents the appearance of ebullition, like borax, basaltic hornblende, &c.

Vegetation.—In some instances a mineral presents the forms of buds, branches, &c. like borax, mesotype, gadolinite, prehnite, &c.

Rounding of the edges, as in talc, &c.

Varnishing, as in some saurolites, &c.

Fritte is when certain molecules are fused and others are not, as in the rock composed of quartz and feldspar; the fritte in this case affords a method of discovering a mixture which otherwise would not be apparent.

Scorification is when the whole mass passes into imperfect fusion, so as not to form one but several globules, as in certain chlorites.

Complete fusion is when the whole mass becomes completely fluid, and forms globules or pearls.

Crystallization.—When a fused mineral assumes upon cooling a regular form, like the phosphate of lead, carbonate of soda, &c.

Changes which attack the form and substance itself.

Combustion or oxidation.—When all the parts of a mineral, or some of them, are combined with oxygen, hence result the following phænomena:

Consumption.—Slow combustion and volatilization of the combustible particles, without flame or smoke, like the anthracite.

Inflammation.—Rapid combustion with flame, like coal.

Dissipation in smoke.—Volatilization of the combustible parts, with visible smoke, which is condensed, and adheres to the cold bodies, like coal, native antimony, &c.

Calcination.—Conversion of a mineral into a metallic oxide in an earthy form: this change may be complete, or only on the surface.

Vitrification.—Conversion of a mineral into a vitreous metallic oxide.

Carbonization.—Conversion of a mineral into charry matter, like coal.

Incineration.—Conversion of a mineral into ashes by the effect of combustion. It may be manifested, either at the surface, as in the kohleblende, at the beginning of the experiment, or in the whole mass, as in the braun kohle.

Reduction is effected by taking away the oxygen from an acid, or other mineralizer, like the carbonate of lead, tin ore, cinnabar, &c.

We ought also to observe the circumstances of the other phænomena which are presented during the course of the experiment:

periment: that is to say, whether they are easy and rapid, or slow and difficult, and simple or compound: they are simple, when one phænomenon only is presented, as in the fusion of compact feldspar; compound, when several are manifested; and the latter may be simultaneous, like the smell, flame, smoke, and carbonization of coal; or successive, like the division, swelling, and fusion of borax.

The change undergone by a mineral may be strong or feeble, as the greater or smaller part of it is attacked. Universal or partial: universal, when it attacks all the particles of a mineral, like native antimony, which is totally converted into smoke; and partial, when it affects only certain particles, as in antimonial silver, when the antimony is reduced to smoke, while the silver remains fixed.

The way in which minerals act is various, according to the methods employed.

When the assay is made with additions, we must consider the nature of the substance added in order to facilitate the fusion of the mineral. Among the fluxes, some operate the reduction and others the oxidation of the metal, as nitre.

The fluxes employed are used either dry, by mixing them with the mineral after reducing both into powder, or by melting the mixture and forming a paste, which is exposed to the action of the flame. Before employing fluxes, it is proper to take the water of crystallization from such as contain it.

The fluxes most in use are, 1. *Minium*, for earthy substances. 2. *Fluor Spar*, which is a very good flux for gypsum, with which it forms an enamel. 3. *Gypsum* is reciprocally an excellent flux for fluor spar. 4. *Borax*, which is often employed indiscriminately for earthy and metallic substances, and which frequently acts as a reductive flux: but before employing it we ought to take care to reduce it to glass. 5. *Nitrated Borax*, i. e. the superabundant soda of which is saturated with nitric acid: this is one of the best fluxes, particularly for metalliferous substances. 6. *Carbonate of Soda*, which is advantageously employed with siliceous substances. 7. *Carbonate of Potash*. 8. *Microcosmic Salt*, which is very efficacious. 9. *Glass of Phosphorus*, i. e. the phosphoric acid reduced to the vitreous state. 10. *Nitre*, which is an excellent assay for inflammable substances, and a powerful flux for metallic substances.

With the exception of nitre and borax, we may employ the various fluxes on all kinds of studs; but with those which possess the property of detonating with charcoal we must use the small platina spoon.

The reductive fluxes are those which take up the oxygen from minerals, or which prevent it from combining with them.

The

The charcoal which serves for the stud fulfils this object: but in order to increase the effect, it must be reduced to powder, and mixed with the mineral, before submitting it to the blowpipe.

Oil is also a very good reductive when applied to the mineral in powder. In the assays with additional substances we observe the way in which the mineral acts with the flux, if it is fusible, or resists fusion. If fusible, we must notice, 1st, if it be tranquilly; 2d, if it be with the disengagement of gas, as in the fusion of gray manganese with borax; 3d, if it be in the form of froth.

We observe what is the time necessary for the fusion, and whether it be quickly or slowly performed; the colour of the flux; and whether it be permanent or fugacious.

The colour may also change when we hold the mineral in various parts of the flame: for instance, the glass of borax, with a slight addition of manganese, is a violet-blue colour while we hold it in the oxidating part of the flame, and it loses this colour when we make it pass into the reductive part.

We observe, also, if a mineral has the property of being reduced, and whether its reduction be complete or incomplete, as in the red ore of copper when treated with borax. Detonation is another phænomenon in the combustion of inflammable bodies, which must be observed.

The products of assays may be:

1. *A glass:* i. e. a transparent or translucid body with a glossy or shelly fracture. We must observe if it be *compact*, *honeycombed*, or *frothy*, as in the *obsidian* and pechstein, or clean and transparent, coloured or colourless.

2. *An enamel:* i. e. an opaque body with a fracture like wax, and the same observations with the above must be made.

3. *A scoria:* i. e. a body generally opaque, or at most translucid, with a surface honeycombed: we observe if its fracture be dull, vitreous, or metallic, and its colour black or brown, &c. We observe if it possesses any polarity, like that of chlorite, some micas, &c.

4. *A fritte:* i. e. a body the fracture of which presents vitreous particles, and others not vitreous.

5. *A regulus:* i. e. a metallic globule.

6. *An ochre,* or earthy substance containing a metallic oxide: we observe its colour.

7. *A coke,* a charry and cellular residue of coal, having much consistence after the combustion of its bituminous part.

8. *A charcoal,* a black, light, friable substance composed of hydrogen and oxydulated carbon.

9. *Ashes*, an earthy, pulverulent, alkaline substance containing molecules of metallic oxides; that is, the residue of the combustion

bustion of various bodies; in colour it is gray or white, yellow or reddish.

10. *Flowers.* A very fine pulverulent deposit, produced by the volatilization of certain substances, and which are attached to the stud, or to some cold body placed above. This deposited matter is sometimes a soot, as in coal, sometimes sulphur, and sometimes a metallic oxide: this last is of various colours: that of antimony and arsenic is white, and that of lead is yellow. The colour is permanent, as in the flowers of antimony; or variable according to the temperature of the oxide, as we see in the oxide of bismuth, which is yellow so long as it is exposed to the flame of the blowpipe, and white as soon as it cools.

Lastly, we ought to observe the changes which the products of the experiments undergo some time after they are terminated, as in the enamel produced by the fusion of barytes, which spreads into small fragments a few hours after fusion.

XCIII. *On the Phænomenon of Arsenic and other Bodies whitening Copper Plates with their Vapour.* By L. V. Brugnatelli*.

When concrete arsenic is thrown upon burning charcoal it emits a white vapour, which may be condensed in a white crust on a plate of copper held over it. This has been considered as an exclusive characteristic of arsenic, and the most convenient practical mode of discovering its presence. An occasion offered of submitting this generally received opinion to the test of experience, by examining the matter contained in the stomach of a young child which was subject to worms and accustomed to take calomel (mercurius dulcis). The concrete matter existing in the fluids of the stomach, being carefully separated, was found insoluble in water, and, when placed on burning charcoal, rose in a white vapour which whitened the surface of a plate held over it. Here there was no suspicion of arsenic, particularly as it was insoluble in water, and instantly recognised by other experiments to be calomel. This mercurial preparation, therefore, whitens a plate of metal precisely like arsenic. Experimenting on several other bodies which produce this effect, I found that the vapour of phosphorus during combustion, that of oxymuriate of ammonia placed over burning charcoal, and the vapours from corrosive sublimate treated in the same manner, all whitened plates of copper; the white spots appear to the eye almost identical; at the moment they cannot be distinguished one from another, and may lead the most experienced observer into error. It is necessary, however, to observe, that the white spots made

* From *Farmacopea Generale.* Pavia 1814.

on

on the plate with the vapour of oxymuriate of ammonia become green in the course of a few days, and that those produced by the vapour of phosphorus endure a very short time till they become brown with the air, and are entirely dissipated. If the white spot produced by the vapour of corrosive sublimate be uncovered, and the white crust entirely rubbed on the plate, the latter then unites in whitening its surface with the original sublimate (*mercurio repristinato*) : the same phænomenon takes place with the crust formed by the vapour of calomel. Hence we have an easy means of discovering the matter which forms a white crust on the surface of copper or brass plates, independent of arsenic.

XCIV. *Observations of* α polaris, *by the Rev. Mr.* L. EVANS, *of the Royal Military Academy, for determining the North Polar Distance of that Star at the Beginning of the Year* 1813.

Royal Military Academy, June 16th, 1814.

SIRS,—HAVING, but a few days ago, seen the Astronomer Royal's most excellent Catalogue of north polar distances of 84 principal fixed stars, and noticing a curious coincidence in the polar distance of α polaris, as determined with the new mural circle made by Mr. E. Troughton, and that of the same star as determined by myself with a transit circle of 24 inches diameter only, made by the same great mechanist; I am induced to request your publication of the whole of my observations and their respective computations, with a view to encourage more attention to the use of transit circles of nearly the same dimensions: for I am inclined to believe that they ought to be appretiated more than they generally are. They require, as in the large instruments, particular precision in their adjustments, prior to any observation, which ought to be taken with great care, calmness, and perfect ease. An excellent opportunity, too, is now afforded for comparison of observations with those in the Catalogue before intimated, for deciding upon their accuracy. And if we take the expense of such small instruments into account, we shall find, that it is not to be mentioned, when compared with the enormous one of the large mural or transit circles, which is far beyond the reach of very many ingenious *astronomical amateurs,* of limited incomes, though they may have it in their power to purchase the smaller ones. Besides, it is my opinion, that the improvement of instruments is not exactly in the direct ratio of their magnitude.

I am, sirs,

Your most obedient servant,

L. EVANS.

To Messrs. Nicholson and Tilloch.

Observations for determining the Polar Distance of α polaris.

α polaris supra polum.

Year of Observations 1813.	Observed Zenith Distance.	Height of Barometer.	Height of Thermom.	Refraction.	Zenith Distance cleared of Refraction.	Aberration.	Solar Nutation.	Deviation.	Precession.	Result of the four Equations.	Mean Zenith Distance for Beginning of the Year.
May 29	36° 48′ 31″·2	29·5	56·5	+41″·88	36° 49′ 13″·08	+16″·51	+0″·42	+6″·65	−7″·96	+15″·53	36° 49′ 28″·61
31	35·6	29·6	62·5	+41·39	16·99	+16·85	0·41	+6·56	−8·08	+15·74	32·73
June 8	38 7	29 38	54·0	+41·93	15·63	+18·12	0·32	+6·59	−8·50	+16·51	32·14
11	32·1	29·45	58·0	+41·53	13·63	+18·50	0·28	+6·58	−8·64	+16·72	30·35
13	30·3	29·62	57·5	+41·94	12·24	+18·73	0·25	+6·59	−8·64	+16·83	29·07
Mean of five observations sup.											36 49 30·58
α polaris sub polo.											
June 8	40 11 44·6	29·35	59·0	+46·69	0 12 31·29	−18·05	−0·32	−6·57	8·50	−16·44	40 12 14·85
10	45·3	29·3	63·7	+46·01	31·31	−18·30	−0·30	−6·58	8·60	−16·58	14·73
11	47·7	29·45	63·5	+46·37	33·07	−18·43	−0·28	−6·58	8·65	−16·64	16·43
12	44·1	29·45	63·5	+46·37	30·47	−18·56	−0·26	−6·59	8·70	−16·71	13·76
13	41·1	29·62	62·5	+46·73	27·83	−18·70	−0·25	−6·59	8·74	−16·80	11·03
Mean of five observations sub......											40 12 14·16
Do.................... sup.....											36 49 30·58
Sum of supra et sub.											77 1 44·74
Difference of do.											3 22 43·58
Half sum of do. or zenith distance of pole.......											38 30 52·37
The half difference or mean north polar distance of α polaris for beginning of 1813............											1 41 21·79
Do. from the observations of the Astronomer Royal											1 41 21·75
Difference											0·04
The latitude becomes known thus:											
Zenith distance of α polaris											36 49 30·58
North polar distance of do.											1 41 21·79
Their sum will be the co-latitude											38 30 52·37
The latitude of my observatory											51 29 7·63

L. E.

XCV. *Chronological Catalogue of Stones and other large Masses, which are presumed to have fallen on the Earth. By* Bigot de Morogues, *of the Mineralogical Society of Jena**.

Yrs. bef. Christ

1451. A shower of stones fell at Gibeon.—Cited by Moses.
654. Stones fell upon Mount Albanus.—Livy.
644. Ditto in China.—De Guigne.
520. A stone fell in Crete in the time of Pythagoras.—Calmet.
467. Ditto in Thrace.—Pliny.
Ditto at Cassandria.—Id.
Ditto at Abydos.—Id.
461. Ditto in the March of Ancona.—Valerius Maximus.
343. A shower of stones near Rome.—Julius Obsequens.
211. A stone fell in China.—De Guigne.
192. Ditto.—Id.
89. Ditto.—Id.
52. A shower of iron in Lucania.—Pliny.
46. A shower of stones at Acilla.—Cæsar.
38. Stones fell in China.—De Guigne.
29. Ditto at Pô in China.—Id.
Ditto at Tchin-Tong-Fou in China.—Id.
22. Ditto in China.—Id.
19. Ditto.—Id.
15. A star fell in the form of rain in China.—Id.
12. A stone fell at Toukouan in China.—Id.
9. Ditto in China.—Id.
6. Ditto at Ning-Tcheou.—Id.
Other stones at Yu.—Id.
A stone seen in the country of the Vocoutins.—Pliny.

Yrs. after Christ.

452. Three stones fell in Thrace.—Cited by Ammianus Marcellinus.
6th century. A stone fell on Mount Lebanon.—Photius.
742. A shower of dust near Edessa.—Quatremère.
823. A shower of flints in Saxony.—Mézerai and Bonaventure de S.-Amable.
852. A stone fell in the Tabarestan.—Quatremère.
898. Ditto at Ahmed-Dad.—Id.
930. Red sand fell near Bagdad.—Id.
From 965 to 971. A stone fell in Italy.—Platina.
Ditto at Lurgea.—Avicenna.

* From the *Journal des Mines*, vol. xxxi. p. 430.

From

Yrs. after Christ.
From 965 to 971. A stone fell at Cordova.—Id.
Ditto in the Djord-Jan.—Id.
998. Stones fell in and near Magdeburg.—Spangenberg.
1071. Balls of earth fell in the Irak.—Quatremère.
1136. A stone fell at Oldisleben.—Spangenberg.
1164. Iron fell in Misnia.—Georgius Fabricius.
1198. Stones fell near Paris.—Henry Sauval.
1249. Ditto near Quedlimbourg.—Spangenberg.
1303. Ditto in the Province of Mortahïah.—Quatremère.
1304. Ditto at Friedberg.—Spangenberg.
1305. Burning stones fell among the Vandals.—Bonaventure de S.-Amable.
1438. Spongy stones fell at Roa.—Proust.
1492. A stone fell at Ensisheim, near Maximilian.—Bartholdt.
1496. Stones fell near Cezena.—Sabellicus.
1510. Ditto to the number of 1200 at Crema.—Cardau.
Commencement of the 16th century. A mass of iron fell between Leipsic and Grimm.—Albini Menische.
1540. Stones fell in the Limosin.—Bonaventure de S.-Amable.
From 1540 to 1550. A shower of iron in Piedmont.—Mercati.
1548. A blackish mass fell at Mansfeld.—Spangenberg.
1552. A shower of stones near Schlensingen.—Id.
1559. Stones fell at Miskoz.—Nic. Ysthuanhi.
1561. A stone fell at Torgau.—Boëce de Boot.
Ditto at Seplitz.—Id.
1564. Stones fell between Malines and Brussels.—Gilbert.
1581. A stone fell in Thuringia.—Chronique de Thuringe.
1583. Stones fell at Castrovillari.—Mercati.
1583. A stone fell in Piedmont.—Id.
1585. Ditto in Italy.—Imperati.
1591. Ditto at Kunersdorf.—Angelus.
1603. Ditto in the kingdom of Valencia.—The Jesuits of Coimbra.
1620. A mass of iron fell in the empire of the Mogul.—D'gehan-Guir.
1627. A stone fell in Provence.—Gassendi.
1635. Ditto at Vago.—Franç. Carli.
1636. Ditto between Segau and Dubrow.—Lucas.
1647. Ditto at Stolzenau in Westphalia.—Gilbert.
From 1647 to 1654. Ditto in the open sea.—Malte-Brun.
1650. Ditto at Dordrecht.—Arnold Sanguerd.
1654. A shower of stones fell in the Isle of Fionia.—Bartholin.
17th century. A stone fell near Copinsha in the Orcades.—James Wallace.

1667.

Yrs. after Christ.

1667. A stone fell at Schiras.—Chladni.
1672. Stones fell at Verona.—Le Gallois.
1674. A stone fell in the canton of Glarus.—Scheuchzer.
1677. Many stones fell near Ermensdorf.—Baldwin.
1697. Ditto at Pentolina.—Phil. Soc.
1698. A mass of stone fell at Waltring in the canton of Berne. —Scheuchzer.
1706. A stone fell at Larissa in Macedonia.—Paul Lucas.
1723. Stones fell at Plescowitz.—Stepling.
1731. Fall of fused metal at Lessay.—Halley.
1738. A shower of stones near Champfort.—Castillon.
1743. Ditto at Liboschitz.—Stepling.
1750. A stone fell at Nicorps.—De la Lande.
1751. Masses of iron fell at Hraschina.—Consistoire d'Agram.
1753. Stones fell at Plaw.—Stepling and De Born.
Ditto at Liponas in Bresse.—De la Lande.
1766. Ditto at Alboretto.—Vassali.
A stone fell near Novellara.—Chladni.
1768. Ditto at Lucé.—Bachelay.
Ditto at Aire.—Gurson de Boyaval.
Ditto in Normandy.—Morand fils.
Ditto near Maurkirchen.—Imhof, Annales de Gilbert.
1773. Ditto at Sena in Arragon.—Proust.
1775. Ditto near Rodach.—Gilbert.
1776 or 1777. A fall of stones at Fabriano.—Chladni.
1779. Stones fell at Petriswood.—Id.
1785. Ditto in the Principality of d'Eichstædt.—Le Baron de Moll.
1790. Ditto in Landes.—Baudin.
1791. Ditto at Cassel-Berardenga.—Philom. Soc.
1794. Ditto at Sienna.—Earl of Bristol.
1795. A stone fell in Yorkshire.—Topham.
1796. Ditto in Portugal.—Southey.
1798. Stones fell at Sale.—De Drée.
A stone fell at Bialoczerkew.—Chladni.
Stones fell at Benarès.—Edward Howard.
1803. A shower of stones at L'Aigle.—Biot.
A stone fell at Saurette.—Laugier.
A fall of stones at Eggenfeld.—Woigt.
1804. Ditto near Glasgow.—Philos. Mag.
1805. Ditto near Doroninsk.—Chladni.
Ditto at Constantinople.—Haïr-Kougas-Ingisian.
1806. Ditto near Alais.—Pagès and d'Hombres Firmas.
1807. A stone fell at Juchnow.—Klaproth.

Yrs. after Christ.

1807. A fall of stones at Weston in America.—Warden.

1808. Ditto at Borgo Santo-Denino.—Guidotti.
Stones fell near Staunern.—Klaproth and Vauquelin.
Ditto near Lissa.—Klaproth.

1809. Ditto on the coast of the United States of America.—Gaz. de France.

1810. Ditto at Charsonville.—Pellieux.

1811. Ditto near Pultawa.—Gaz. de France.
Ditto at Berlanguillas.—Id.

1812. Ditto in the environs of Grenada (near Toulouse).—Moniteur.

Masses presumed to have fallen on the Earth.

Iron as mentioned by Scaliger.
Stone, which forms part of the collection of De Dree.
Mass of native iron seen in Siberia by Pallas.
Mass of iron at Otumpa seen by Rubin de Celis.
Another mass of iron seen in America by Ditto.
Native iron seen in several parts of Mexico by Humboldt.
Ditto of Durango and Zacatecas.—Ditto.
Ditto at the Cape of Good Hope.—Smithson Tennant.
Ditto of Senegal seen by Adamson.
Ditto at Aken by Lœber.
Ditto in Bohemia mentioned by Born.
Masses of iron found near the Red River in Louisiana.—Gibbs.

XCVI. *Description of a Sofa invented by Mr.* Samuel James, *Surgeon, Hoddesdon, Herts, for the Use of Persons confined to Bed by Fractures or other Causes.*

Professional men have long regretted the want of a machine to assist the practitioner in the proper treatment of afflicted persons, who, from fractures or other causes, may be confined to their beds, without any power of locomotion, or any substitute for it.

Among the numerous improvements of the present age, the public will be highly gratified to learn that this useful desideratum has been at length accomplished by Mr. S. James, Surgeon, of Hoddesdon, Herts. After numerous trials, this is found to be the most complete machine ever invented for the relief of mankind; combining the ease of a bed with a mechanical substitute for locomotion.

The

The invention appears to have long occupied the humane attention of Mr. James. Seventeen years since, an account of a machine invented by this gentleman for fractures of the legs and thighs appeared in the *Encyclopædia Britannica*, which is still highly approved of by the faculty in general.

Attestations to the great utility of the present invention have been given by the highest authorities in medicine and surgery. The following letters form but a small part of the flattering testimonials with which the inventor has been honoured.

"Lincoln's Inn Fields, July 31, 1813.

"Dear sir,—I have carefully inspected the drawing of the sofa, which you have invented for patients who are incapable of moving by their own exertions.

"It appears to me that this invention is very superior to any other that I am acquainted with, for such purposes. The simplicity of its construction, and the ease with which the body and limbs can be moved into the most favourable positions, must greatly contribute to the comfort of the patient.

"I remain, dear sir, yours faithfully,

"Henry Cline.

"*To Samuel James, Esq.*
Hoddesdon, Herts."

"Russel Square, Nov. 15, 1813.

"I have attentively examined the sofa so admirably contrived, and so ingeniously constructed; the discovery of Mr. Samuel James, surgeon, at Hoddesdon.

"It facilitates the motion of the whole body, or any distinct part of it, without any exertion on the side of the invalid, and without any painful effort; it is therefore calculated to afford the greatest relief in cases of gout or rheumatism, in fractures, and all other external injuries where surgical aid is necessary. It keeps the diseased parts in the most tranquil and relaxed situation: it preserves them in a steady and uniform position, so that external injury is avoided. It has, likewise, the advantage of being so commodious and accommodating, as to assume the appearance of an elegant sofa.

"(Signed) Wm. Saunders, M. D."

"Aldermanbury, Nov. 18, 1813.

"My dear sir,—It must be known to every person engaged in the management of the sick, that the instances are innumerable in which it is of the greatest moment, as in severe affections of the chest, compound fractures, &c. that the patient should be furnished with the means of having his body easily placed,

placed, and maintained in that position which the nature of his illness requires: and from the drawings which you did me the favour to show me when you were last in town, I am of opinion that a couch or sofa, made on the construction you propose, is well calculated to answer the intended purpose; and will be the more generally useful, from the facility with which its machinery can be transferred to a common bed or sofa.

"I am, dear sir, yours very truly,

"WILLIAM BABINGTON.

"*To Samuel James, Esq. Surgeon,*
Hoddesdon, Herts."

"New Broad Street, Nov. 26, 1813.

"My dear sir,—The sofa bed which you have sent me a drawing of, is a highly ingenious invention, well constructed to move and support those who from long-continued disease have lost the use of their limbs; and admirably designed to give a good position to fractures, more especially in that most difficult case to manage well, the fractures of the thigh.

"Few, who are occupied in business as you are, give themselves time to think of such improvements; and I therefore consider this invention as highly creditable to your character and talents.

"I am yours very truly,

"ASTLEY COOPER.

"*To Samuel James, Esq. Surgeon,*
Hoddesdon, Herts."

Plate VII. will enable the reader to judge of the capability of this sofa to administer ease to the sick and infirm, and to facilitate the cure of fractured limbs in particular.

In all cases of palsy, asthma, consumption, dropsy, rheumatism, gout, fractures, and distressing debility, or that excessive languor which is sometimes caused by severe fatigue, and at other times is the consequence of long continued illness, great relief will be received from the use of this invaluable machine, which is manufactured under the inspection of the inventor, by Prentice and Son, Little Wild Street, Lincoln's Inn Fields, where specimens are exhibited for inspection and trial.—The machinery is so constructed that it may be readily transferred to a common bed.

XCVII. *Notices respecting New Books.*

A Treatise on the supposed hereditary Properties of Diseases, containing Remarks on the unfounded Terrors and ill-judged Cautions consequent on such erroneous Opinions; with Notes illustrative of the Subject, particularly in Madness and Scrofula. By Joseph Adams, *M.D. F.L.S. of the London College of Physicians, &c.*

This inquiry is equally novel and important. Hitherto one class of philosophers has shrunk from it with a kind of superstitious awe, while another has been deterred from ever spending a thought on the subject, by the vulgar disgusting manner in which it has been viewed by some foreign writers. Yet there is perhaps no physical inquiry more intimately allied with morality, of which all persons consider themselves competent judges. Many of the diseases called hereditary are rather the result of similarity in moral and physical education, of imitative habits, than of any physical organization. Others depend on local climate, disparity of age, intemperance either in regimen or exercise, or erroneous theories. To ascertain the constitutional from the accidentally transmitted diseases, Dr. Adams makes the necessary distinctions between a *family* and an *hereditary* peculiarity of constitution, the former being confined to a single generation, to brothers and sisters of the same parents, while the latter is traced from generation to generation. "Diseases either appear at birth, and are called *congenital* or *connate*, or they arise afterwards: the first only can with propriety be called *hereditary* or family diseases; all others should be considered as hereditary or family *susceptibilities* to certain diseases. The degrees of *susceptibility* should be distinguished by appropriate terms. If the *family* or *hereditary susceptibility* is such that the disease, though not existing at birth, is afterwards induced without any external causes, or by causes which cannot be distinguished from the functions of the œconomy, such a state may be called a disposition to the disease. But if the susceptibility, though greater than is remarked in other families, is so far less than a disposition as always to require the operation of some external cause to induce the disease; this minor susceptibility may be called a predisposition to the disease. Connate or congenital diseases are more commonly family than hereditary; some of them, being mortal, as connate hydrocephalus, cannot be transmitted; other connate peculiarities are more properly organic privations or imperfections, as connate deafness or connate cataract. Dispositions are found in some families to diseases

eases which are connate in others: hydrocephalus, which is connate in some families, in others occurs to several brothers and sisters in succession as they arrive at a certain age. The disposition to blindness and deafness is often hereditary, though the connate privation of these senses is generally confined to a single generation. When the disposition is hereditary, the children are born with perfect organs, but usually about the age of puberty their vision declines. Predispositions also are found in some families, and dispositions in others, to diseases of the same organs, and called by the same name, as pulmonary consumption. In some families, a number of brothers and sisters fall into consumption on arriving at a certain age: this may strictly be called a *family disposition* to the disease, inasmuch as it is confined to a single generation, and as we can discover no external cause to excite it. Another kind of consumption, and the most common in cold climates, is hereditary; but only in *predisposition*, always requiring the influence of climate to induce it, and consequently always to be prevented, and often relieved, by avoiding the exciting cause. Gout and madness are, by almost universal consent, deemed hereditary; yet, if we admit the general implication as to their immediate causes, both these diseases, and particularly the former, should be considered as only hereditary in predisposition."

Dr. A. illustrates these accurate distinctions by well authenticated cases, which must contribute to diffuse a more correct knowledge of such diseases into the language of popular conversation, as well as tend to remove erroneous prejudices, and many serious obstacles to social happiness. From his extensive medical practice, however, and his acute observation, we expected a curious section on family cutaneous diseases, which, in an age when external appearance is one of the most general studies, could not fail to interest great numbers of his fashionable readers. The remarks on elephantiasis occasionally digress from principles to persons. The author's concluding summary, indeed, is worthy of his talents and medical skill. The result is,

"That *connate* diseases or privations are *not* hereditary; that *dispositions* to certain diseases are more commonly family than hereditary; that the diseases arising from them usually show themselves at certain ages; if early in life, that we have little chance of preventing or curing them, but that such of the children as escape that age, are as safe as the descendants from other families. That *hereditary predispositions* to the most prevalent diseases are brought into action either by climate, which destroys at an early age those who would be the means of transmitting such predispositions to posterity, or by such external causes as may often be prevented. That whenever an

hereditary or *family susceptibility* to any disease is suspected, the changes in the constitution induced by gestation, parturition, and the more advanced climacterics, should be particularly attended to. That if the human race, like other animals, has a constant disposition to restore itself from every irregularity, the divine law, which forbids any sexual intercourse between near relations, seems sufficient to correct every peculiarity unconnected with climate. That if an *hereditary disposition* is generated by climate, it must progressively increase from the constant operation of such combined causes. That no remedy, therefore, can be sufficient, but the prevention of propagation as soon as the disposition becomes hereditary, and that such provision is made by the diseased action itself. That as far as our inquiries into these irregularities have hitherto extended, sufficient provision is made for correcting them by the influence of climate, by the interdiction of marriage between near relations, and by the effects which the irregularities themselves induce. *That all interference*, therefore, *with the dictates of nature, beyond the expression of revealed will, appears unnecessary*. Finally, that to lessen anxiety, as well as from a regard to the moral principle, family peculiarities, instead of being carefully concealed, should be accurately traced and faithfully recorded, with a delicacy suited to the subject, and with a discrimination adapted to the only purpose for which such registers can be useful."

The practical wisdom and good sense of these conclusions must be obvious to every reader. Dr. A. in the notes has also some judicious remarks on goitre, which he considers a family disease. An instance tending to confirm this opinion may be given. In a valley near Guadir, in Granada, goitres appear: in an adjoining one watered by the same river, and subject to the same winds, no traces of them are found. The family descent of calculous diseases might also be traced; but Dr. A. who has so well begun the inquiry, under the enlightened auspices of the worthy President of the Royal Society, will doubtless pursue it.

The second volume of the Transactions of the Geological Society will be ready for delivery to the members early in July.

A Manual of Mineralogy has been published by Mr. Arthur Aikin, Secretary to the Geological Society.

M. Mionnet of Paris, has published a description of ancient Greek and Roman medals, with a table appreciating their scarcity and value. It consists of six volumes in octavo, and one volume of plates, and comprises a series of 20,000 impressions taken in sulphur.

XCVIII. *Pro-*

XCVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 9 and 16. A LONG and interesting paper, by Sir Humphry Davy, was read, detailing this philosopher's experiments in Italy, on the combinations of iodine with the alkalis, potassium, sodium, hydrogen, &c.

June 23. Another paper by Sir H. Davy, sent from Rome, was read, containing an account of his experiments on diamonds and carbonaceous matter, performed at Florence and Rome with burning lenses. Sir H. having conjectured in his third Bakerian Lecture that the diamond owes its peculiar characters to a small portion of oxygen, availed himself of an opportunity while at Florence to operate on this substance with a very powerful lens and the concentrated rays of the sun, instead of the Voltaic pile. He made a variety of experiments on the combustion of small diamonds laid in a platina cup and placed in a glass globe, through which the solar rays were made to pass and burn the diamonds; but in none of them was there any oxygen evolved: whence he was induced to abandon the idea of oxygen forming any part of the diamond. He next directed his attention to ascertain whether, according to the opinion of Guyton Morveau, hydrogen or water might not exist in diamond; but the result was similar, no trace of either appearing. Moisture, indeed, in his first experiments was discovered; but it was entirely owing to an imperfection in the apparatus, which was afterwards remedied. Charcoal was then submitted to similar experiments, and emitted some hydrogen. Hence Sir H. concludes that diamond is perfectly pure carbon, and that its hardness and transparency are derived from its crystallization, and not from the admixture of any other elementary body.

Sir H. has examined six different species of sea-weed on the coast of the Mediterranean, without finding any iodine in them, except some very slight traces. He also suggested some improved methods of detecting this singular substance; and described a number of its combinations and appearances with muriatic and other acids, as well as with hydrogen and the gases.

Smithson Tenant, Esq. furnished a paper giving a description of a cheap and easy method of preparing potassium in considerable quantities. The author, after mentioning the different methods hitherto adopted to prepare this metal, stated his own, which consists in an improvement of Gay-Lussac's application of a gun-barrel. Instead of having a peculiar furnace and different gun-barrels, he merely takes two iron tubes, one considerably wider than the other: the wide one is filled with iron filings and potash,

potash, and one end of it is welded together: in the other is inserted a smaller tube perfectly air-tight, and destined to collect and retain the potassium. The apparatus thus adjusted, the wide tube is placed in a furnace, smith's forge, or any powerful fire, when the potassium is sublimed, rises into the small tube, and is there collected pure and fit for use.

Dr. Kidd communicated to the Society a short paper on the Formation of Nitre on the Walls of the Laboratory in Oxford. The walls are composed of a calcareous stone containing some shells, and the nitre forms on its surface in the greatest quantities where the walls are below the surface of the ground. It appears that the atmosphere assists its formation, as Dr. K. placed a glass over a part of the wall which yielded the nitre in greatest abundance, when the rapidity of its production was greatly impaired. The nitre produced is tolerably pure.

Sir Everard Home, Bart. read a paper containing a description of some fossil bones found in Dorsetshire, and now in Mr. Bullock's museum.

Sir E. has examined these fossil bones with great attention, but without being able perfectly to satisfy himself to what particular species of animal they have belonged. The teeth he at first conjectured might be those of the crocodile, but on more mature examination he discovered that they could not possibly belong to that animal; the same uncertainty exists respecting the jaws and skull, which he determined not to be those of the shark genus. The non-existence of the elephant, hippopotamus, &c. in this country he considered as no argument why some of the bones found in Dorsetshire should not have belonged to those animals; but the depressions which the bones have experienced, rendered it impossible for him to say positively to what species of animal they had belonged. Sir E.'s paper was illustrated by drawings of the different fossil bones alluded to.

PROCEEDINGS OF THE FRENCH INSTITUTE FOR THE YEAR 1813. BY M. CUVIER.

[Continued from p. 311.]

In our analysis for 1811, it has been seen how by accelerating evaporation in vacuo, and by the presence of a strongly absorbent body, Mr. Leslie of Edinburgh succeeded in freezing water at all times of the year. An apparatus which he subsequently invented has been exhibited to the Class by M. Pictet; and our colleague M. Gay-Lussac, in repeating the experiment, recalled a well-known fact connected with the same subject, namely, that cold is produced in certain machines from which we

we allow condensed air to escape: he has also proved that at all times of the year it is sufficient that the air should be c ndensed in a two-fold degree, in order to obtain ice, and he thinks that we may procure it easily in warm countries by condensing the air by means of a fall of water.

We may, by employing bodies more evaporable than water, attain degrees of cold truly astonishing, and freeze not only quicksilver but the purest alcohol. M. Configliacchi of Pavia has frozen mercury by the evaporation of water alone. It was thought that this pressure of the air, the influence of which is so powerful in retarding the evaporation of liquids, also retarded the solution of salts, or, what comes to the same thing, accelerated their crystallization when they were dissolved; and in fact a saturated solution of Glauber salts, or sulphate of soda, which preserves its fluidity when it is cooled in vacuo, also crystallizes when we admit air to it. But M. Gay-Lussac says that this does not happen to all salts indiscriminately; and even with respect to the sulphate of soda, the phænomenon is not occasioned by the circumstance alleged. When we intercept the contact of the air, by a stratum of oil for instance, the crystallization is retarded as when we suppress its pressure by making a vacuum; whereas, on the contrary, the pressure of a column of mercury in no respect accelerates this crystallization. A solution which passes through mercury, from which the air has been driven by ebullition, does not crystallize, and if it passes through common mercury it coagulates instantly. Agitation, the introduction of a small crystal, and many other causes, produce the crystallization, whatever be the degree of pressure. Thus M. Gay-Lussac concludes, that it is not by its pressure that the air diminishes the dissolving power of water. He affirms also, that it is not by absorbing air that water loses this power; but he thinks that it is a phænomenon more or less analogous to that of pure water, which, as is well known, remains fluid at some degrees below its real freezing point, when we prevent it from being shaken; but it freezes the instant we give it the least shake.

The most evident source of heat in the world is the sun's rays. But it has been long remarked that these rays divided by the prism do not all give an equal heat; and M. Herschel, the celebrated astronomer, ascertained some years since that their power of heating went on augmenting from the violet to the red: he even ascertained that outside of the spectrum there were rays which, without being luminous, possessed a heating property more powerful than that of the red rays. Messrs. Ritter, Beckmann, and Wollaston announced soon afterwards that the power of the luminous rays to produce certain chemical changes

changes is distributed in an inverse ratio, and is exercised particularly in the violet ray, and beyond this ray.

M. Berard, a young chemist of Montpelier, who repeated with much precision both sets of experiments, ascertained their exactitude in several respects: he even found that the chemical power of light diminishes in proportion as we approach the middle of the spectrum, and that it vanishes beyond it. But according to him, it is at the extremity of the red ray that the maximum of the heating power resides, and outside of the spectrum it diminishes. M. Berard has also ascertained that these properties belong to the light reflected by ice, and to that which has been divided by the Iceland spar, as well as to the direct light.

Decisive results have not yet been obtained on the power of magnetizing iron, ascribed to the violet-coloured rays by M. Morichini, an ingenious Italian chemist. Although the magnetic needles exposed to these rays were apparently affected under certain circumstances, they exhibited no signs whatever in others, without our being able hitherto to account for this difference; for, in both cases, all other causes known to produce polarity had been carefully removed. It is true that the summer of 1813 had not favoured this kind of inquiry, on account of the unsettled weather.

Of all the phænomena which heat presents, the dilatation which it produces in bodies is that of which the laws admit of being expressed most naturally by mathematical formulæ; and the knowledge of these laws, which forms an essential part of physics, is also very important in a host of chemical experiments. M. Biot has been considerably occupied with this subject; and taking the dilatation of mercury as a term of comparison, he finds that the real dilatation of other liquids may always be obtained by the sum of this dilatation, of its square, and of its cube; multiplying each of these three terms by a particular coefficient, which we must determine for every liquid, but which being once determined remains the same at all degrees. As the substance of the thermometer which contains the liquid under examination dilates also, the apparent dilatation is different from the true: nevertheless, M. Biot demonstrates that it takes place according to a similar law. He calculates afterwards, according to the experiments of M. De Luc, the coefficients adapted for eight of the liquids whose laws it is most necessary to know; and shows that, these coefficients once obtained, its formula gives the dilatation of each degree equally well with the experiment. Lastly, he has made the application to the combined dilatations of the vase and of the liquid, and has shown that

that we may separate the effects which belong to the liquid and to its envelope, and appreciate their influence with sufficient accuracy to recover by calculation alone all the results observed; so that calculators may in future dispense, in an infinite number of cases, with immediate observation, and we may introduce with confidence its data into the elements of phænomena. This is an advantage of the more consequence, as these kinds of researches are extremely delicate; and if we do not pay the greatest attention to them, a multitude of trifling causes will disturb the observer.

A dispute has long existed among chemists, as to the precise moment at which alcohol was formed in wine. The greater number formerly thought that alcohol, or spirits of wine, was an essential product of fermentation; but M. Fabroni has maintained a contrary opinion. According to him, it is only accidentally, and when it excites too much heat, that fermentation engenders alcohol; but in common wines, the alcohol is produced merely by the heat applied for their distillation; and the chief proof which he gives of this is, that we cannot extract it from these wines by potash, although the latter exposes the slightest particle of alcohol which we introduce on purpose. M. Gay-Lussac has adhered to the old opinion, by showing that potash also demonstates the alcohol which is natural to wine, when we previously cleanse it by litharge from the principles which surrounded it and opposed its separation; and that we may obtain this spirituous liquor by distilling wine at a temperature of 15 degrees, which is far inferior to that of the common fermentation. M. Gay-Lussac, to avoid all adulteration, made the wine himself upon which he made his experiments, and found alcohol in it as well as in the other kinds. He has also shown that we may obtain the pure alcohol of Richter by employing quicklime, or rather barytes, instead of muriate of lime.

[To be continued.]

XCIX. *Intelligence and Miscellaneous Articles.*

On the 13th of June 1814, the first Class of the French Institute (being that for the encouragement of mathematics and the physical sciences) held a meeting, when a paper was read with the following title: "Account of a new Heliometer, intended to give the precise measurement of the diameter of the Sun, by Alexis Rochon, member of the Institute, and of the Academy of St. Petersburgh." After stating that the subject had been propounded as a prize dissertation by the Petersburgh Academy for last year, M. Rochon proceeds: "In the *Moniteur*

for

for 1812 will be found the description of a micrometer of rock crystal, which I presented to the Institute, not only for taking the diameters of the sun and moon, but also intended to be used in difficult problems in naval tactics and geodosy. Not being fully satisfied of the degree of precision which I obtained in the measurement of the sun, I became desirous of destroying, in the two solar images, the aberration occasioned by the unequal refrangibility of the rays which traverse crystallized substances in which we develop the effects of double refraction: for this reason I renounced the idea of becoming a candidate for the Petersburgh medal.

"I owe it to my colleague Arago, to acknowledge that from him I first learnt, that the old pieces of stained glass in our churches possessed the astonishing property of transmitting homogeneous rays only when they were painted with the oxide of copper. These pieces of stained glass are either of a beautiful red or bright green; for according as the painting is more or less *fired* (*cuite*) the oxide assumes these colours; although they are very different; and it was by using glass of a red colour that I finally attained the achromatism necessary for the precise measurement of the sun's diameter.

"The Memoirs of the old Academy of Sciences prove that M. Monge was the first to make the observation, and subsequently M. Hassenfratz affirms that he made the same remark on painted glass which was stained of a green colour by the oxide of copper. This second remark will prove very useful to me, if it produces the same effect with glass painted red by means of the same oxide; and I beseech Messrs. Vauquelin and Thenard to assist me in my new inquiries. It is with a piece of glass painted red with the oxide of copper, that I obtain at this instant two images of the sun perfectly well defined, and henceforward I have the means of increasing at pleasure the effect of double refraction. Artists who are employed to cut prisms of this substance, in order to procure heliometers for astronomers, ought to proceed in the following manner. They will select very pure and finely crystallized specimens. The cube will not give in the direction of the axis any sign of double refraction; but this cube when cut transversely to the axis will give two prisms, which will have attained the *maximum* of double refraction. A second cube similar to the first, but cut in an opposite direction, will produce an effect similar to the first, with this very remarkable difference, that by putting them close together in opposite directions the effect of the double refraction will be doubled: in this way we shall procure, with these two cubes, two new cubes producing a quadruple refraction. Thus, by multiplying the cubes, we shall increase at plea-

sure,

sure, and indefinitely, the effect of the property of double refraction in those crystallized substances which present this incomprehensible phænomenon.

"We are indebted to the late M. Malus for an entirely new process for discovering in substances, whatever may be the alteration from their primitive form, their axis of crystallization. The instrument which he contrived for this purpose is so ingenious, that it ought to be engraved on his tomb, like the cylinder inserted within the sphere, which enabled Cicero to discover the tomb of Archimedes in Sicily. It is best for the measurement of the sun, which we know to be about 32 minutes, to employ in my heliometer three or four cubes, shaped as I have described, in order that this micrometer may be very close to the focus of the eye-glass, without altering in a sensible manner the goodness of the object-glass, and its dimensions not exceeding that of the diameter of the eye-glass. The importance to astronomy of the precise measurement of the diameter of the sun made me desirous that my colleague Arago should verify the heliometers which I sent to the Royal Observatory. This eminent astronomer has already verified, by numerous observations, the diameter of the principal planets by the microscope, on which the heliometer which I have now described is absolutely calculated."

Mr. Sowerby is making a sword of meteoric iron, to present to the Emperor of Russia.

A very interesting discovery of ancient medals has been recently made in the department of Jura, in the Alps. A boy who was feeding sheep having ascended a very high rock, struck his stick against it, when to his surprise it entered easily. Having called the attention of some of his companions to the circumstance, they dug into the aperture, and discovered a pot half zinc and half copper of the capacity of about two pints. It was filled with copper medals edged with silver, bearing the effigies of various emperors, of excellent workmanship. Several have legends and exergues of various kinds, and all were covered with verdigrise. They are of the reign of Dioclesian, Constantine, Maximinus, &c. The form of the pot which contained these medals is antique: it is contracted greatly at the upper part, its colour is whitish, but it exhibits neither inscriptions nor engravings.

On the 22d of May, at half past 11 A. M., a shock of an earthquake was perceived at Oleron, in the South of France. "Never," says an eyewitness, "in the memory of the oldest inhabitants

habitants was any former shock so violent or of so long duration. It appeared that an eruption followed by a thick and black smoke burst from the mountain of Louvie, three leagues from Oleron. Some large rocks were detached from the mountain, and their fall killed five cows and demolished a house. The second shock was so violent that several houses at Gand, on the road to Pace, were destroyed. Here a great number of chimneys were thrown down, and terror filled every breast. The churches were in an instant crowded with the flying inhabitants, and a young girl was crushed to death in the attempt to gain an asylum."

A similar shock was perceived at Marmand on the same day and at the same hour. It was from west to east, and was preceded by a clap of thunder in the west, accompanied by large black clouds. The shock lasted two seconds.

Some travellers recently arrived from Wallachia have brought an account of a terrible calamity which has befallen the inhabitants of Oybestein. This district, one of the most populous in the country, was situated in the neighbourhood of several lofty mountains; some of these were cultivated to their summits, and the sides were covered with the dwellings of the natives; the base of the highest, however, is supposed to have been sapped by the long rains. On the night of the 20th of April, while the inhabitants, unsuspicious of such a calamity, were buried in repose, the peak called the Devil's Neck descended with a noise resembling an earthquake, and overwhelmed in its progress houses, forests, and innumerable cattle. The concussion was so frightful, that the inhabitants of the adjacent villages started from their beds, and were seen running naked from their habitations to seek safety in the plains. The extent of this calamity had not been ascertained; but it was supposed that 400 persons had been buried beneath the ponderous fragments, which extended and covered a mile of ground. The general distress was much increased by the groans which were heard issuing from the ruins four days after the avalanche.

M. Sergel, the celebrated Swedish sculptor, died lately at Stockholm at the age of 74. He had resided nearly twenty years at Rome, and was a member of the Academy of Painting and Sculpture at Paris, and of the French Institute. His principal works are the groupe of Psyche and Love, and that of Mars and Venus, the monument erected to Descartes in one of the churches at Stockholm, and the statue of Gustavus III. placed near the palace. M. Sergel has left several pupils behind him, one of whom (M. Bystroem) has obtained a pension to enable him

him to travel in Italy. It was upon M. Sergel's suggestion that Gustavus purchased the Endymion, one of the *chefs d'œuvre* of antiquity at Rome, and which now forms the chief ornament of the Stockholm museum.

M. Le Gallois, of the Faculty of Medicine of Paris, has lately made some interesting inquiries on the principle of life, and particularly on the motion of the heart as connected with it. He was led by an adventitious circumstance to consider how long the young of rabbits can live without respiring, immediately after their separation from the mother, before the natural period of utero-gestation terminates. He found the time to be variable, and greater in proportion to its proximity to the termination of pregnancy. He then attempted to discover, how long these animals can live after decapitation; and found this to be also variable according to the age of the animal: but he likewise observed, that it is always precisely equal to the time during which the animal resists suffocation, or takes in dying by asphyxia. M. Le Gallois thence concluded, that decapitation only destroys animals by suffocating them; that is, by impeding the respiration necessary to their existence.

The analogy being once assumed, required to be proved by direct experiments. There was, besides, this difference between the effects of simple asphyxia and decapitation; viz. the animal under asphyxia made vain efforts to breathe, whilst in that decapitated all the motions of respiration were destroyed. It became requisite to discover the cause of this difference. To resolve the first question, M. Le Gallois endeavoured to supply the material of respiration in the decapitated animal, by inflating the thorax, after having tied the arteries: this experiment succeeded. Sensation and voluntary motion were seen to return with inflation; they were of various duration in different rabbits, but even in the youngest continued for several hours.

As it was thus proved that the destruction of the brain occasioned death by the interruption of respiration, it became necessary to inquire whether the principle of the motion resided in this viscus generally, or was confined to one of its parts? For this purpose, our experimentalist opened the cranium of a young rabbit, and removed the brain by successive portions, cutting it horizontally, from before, backwards. He found that all the cerebrum could be thus removed, and the whole of the cerebellum, and even part of the medulla oblongata, without interrupting respiration; but this function suddenly ceased when the origin of the eighth pair of nerves was included in the slice cut from the medulla oblongata. It therefore became evident, that the principle of motion in the respiratory organs proceeds

proceeds from this point: in fact, respiration is no longer performed when these nerves *only* are divided, without injury to other parts, and the animal dies from asphyxia, accompanied by some peculiarities which M. Le Gallois has noticed.

We are told that the total removal of the medulla oblongata in a rabbit, decapitated and revived, instantly kills it. If the same operation be performed on one which has not been so circumstanced, and in which the brain is perfect, it dies, although not in the same manner; the *trunk of the body* is *instantly* deprived of life; but the head continues to live a short time, and is proved by a gaping (*baillement*) indicative of efforts made by the animal to carry on the function of respiration.

If a rabbit be divided transversely into two equal portions, each of these continues to live separately during a certain time, which is shorter or longer according to the age of the animal; and longer the younger it is. Each of these parts feels and acts by itself: and they also die separately as soon as their respective portions of spinal marrow are destroyed. From this view of the subject, it is evident there are two centres of vitality, or rather two sources of distinct sensations. The life of the whole trunk depends on the spinal marrow, and the life of each portion of the trunk on the portion of spinal marrow which gives off nerves to it: besides, it so completely and entirely depends on this cause, that after the heart, liver, intestines, and internal organs of the animal have been removed, it continues to survive as long as the portion of spinal marrow which animates it is left entire.

Since it is not possible to remove the whole of the head of a warm-blooded animal, and leave the medulla oblongata in continuity with the spinal marrow, without dividing many considerable blood-vessels, the loss of blood from which greatly lowers vitality, experiments were made on some cold-blooded animals, such as salamanders. The wound caused by decapitation cicatrised. and they continued to exist until life was exhausted by simple want of nutrition.

These united experiments prove that the maintenance of life in any part of an animal essentially depends on two circumstances; one of which is the integrity of that portion of the spinal marrow corresponding with the part; the other, the continuance of the circulation of arterial blood in this part, an effect produced by respiration: it follows then, that any portion of an animal can be made to exist separately (*isolément*) so long as both these conditions can be fulfilled.

M. Le Gallois supposes that the brain wills and regulates all the animal motions, but the movements themselves depend on the influence of the spinal marrow. A cold-blooded animal, for example,

example, lives for some days, and moves its limbs after the brain is removed; but its movements are useless, and those of the feet in contrary directions, so that, if it takes one step forward, the next is perhaps made backward. He imagines that the spinal marrow is acted upon by the brain, in the same manner as the muscles are acted upon by the spinal marrow.

The results of M. Le Gallois' physiological labours were submitted, by the National Institute, to Messrs. Humboldt, Hallé, and Percy, by whom a particular examination of the facts was made. The Doctor repeated all the experiments in their presence with complete success; and their report is highly creditable to the talents of the ingenious author. The subject is one of great interest: and we should be glad to see it investigated on this side of the Channel.

ABSENCE OF MAGNESIA IN HUMAN BONES.

According to the latest experiments made on human bones, by Hildebrandt, the analysis of Fourcroy and Vauquelin is confirmed, that they do not contain any oxyphosphate of magnesia, as maintained by Berzelius in his Animal Chemistry.

A nervous fever prevails at Basle, in Switzerland, which attacks young persons, and has assumed a character of malignity. In four or five days the patient expires, and the disease has hitherto proved fatal in every instance.

In May last the plague raged violently at Smyrna, Odessa, and in several islands of the Archipelago. It had also made its appearance at Constantinople, but not to such an extent.

The university of Wilna, in the Russian empire, has resumed its former rank among the learned institutions in Europe, and a veterinary school has been added to its other lectureships. The number of students is from 6 to 700 annually.

The universities of Halle and Berlin have been restored to their former rank under the auspices of the King of Prussia. The Saxon students have preferred visiting the latter school, where 49 professors are to give lectures in the course of the summer.

The celebrated Dr. Sparzheim, the colleague of Dr. Gall, being now in London, purposes to give a Course of Lectures on the Physiology of the Brain, at his Rooms, No. 11, Rathbone-place, where further particulars may be learnt of him. The Lectures are to begin on the 11th of July.

Meteorological Observations made at Clapton from the 9th to the 20th of June 1814.

Since the date of my last observations I have not kept regular accounts of the weather till the 9th of June: the weather in the interval was chiefly cold, the prevailing winds easterly, and a great deal of cloudiness.

June 9.—Easterly wind; clear sky, and hot in the sun, with *cirrus*, &c.

June 10.—Clear and cloudy by intervals; air warmer.

June 11.—Fair day; various clouds. Easterly wind, and a breeze.

June 12.—Cloudiness and rain in the morning; fair afternoon. Wind southward.

June 13.—Much warmer, with gentle showers.

June 14.—Very hot day. Thermometer 84° in the shade; much *cirrus*, *cumulostratus*, and that dense white feature of *cirrocumulus* which precedes storms. I predicted from it the storms of next morning. SW. and variable.

June 15.—Early this morning a thunder shower; warm day after the rain, with light gales from south, and *cumuli*, &c.

June 16.—Warm but showery; various clouds, as in such weather.

June 17.—Fair, warm day; various clouds and showers. SW.

June 18.—Warm and gentle showers. Southerly.

June 19.—Rainy morning; fair evening.

June 20.—Fair, but a great deal of cloud, and rather cooler than yesterday.

The weather for the last ten days has been mild, pleasant and wholesome, with showers at intervals, which have contributed much to complete the vegetation this backward spring. The Monkshood Poppy (*Papaver orientale*) and the *Tragopoga polyfolium et pratense*, came into flower later than usual. The clouds have been such as usually accompany summer showers; a continual tendency to *cumulostratus* and *nimbus*, with *cirrocumulus*, *cirrus* and *cirrostratus* of the common sort in the fine intervals.

Clapton,
June 21, 1814.

THOMAS FORSTER.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For June 1814.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
May	27	45	58	51	29·80	54	Cloudy
	28	52	65	55	·60	66	Fair
	29	58	64	47	·78	80	Fair
	30	56	67	52	30·10	76	Fair
	31	54	66	48	·02	64	Fair
June	1	47	60	47	29·92	60	Fair
	2	48	56	46	·87	0	Foggy with rain.
	3	51	54	45	·75	0	Rain
	4	50	52	48	·82	0	Rain
	5	50	51	46	30·00	27	Cloudy
	6	50	52	47	·01	38	Cloudy
	7	46	51	44	·02	37	Cloudy
	8	49	54	46	·04	39	Cloudy
	9	45	59	47	·06	66	Fair
	10	52	63	48	·05	60	Fair
	11	54	62	53	29·92	69	Fair
	12	55	70	56	·76	60	Fair
	13	57	70	56	30·05	72	Fair
	14	68	79	67	29·92	86	Fair (thunder at
	15	60	70	56	·85	57	Cloudy night)
	16	56	69	54	·92	46	Showery
	17	55	64	53	30·08	53	Cloudy
	18	54	60	52	29·96	36	Showery
	19	56	56	51	·85	25	Rain
	20	51	57	52	·75	35	Cloudy
	21	54	58	51	·80	40	Cloudy
	22	51	57	53	30·02	43	Cloudy
	23	52	57	51	·26	44	Cloudy
	24	51	55	51	·31	36	Cloudy
	25	50	56	54	·28	35	Cloudy
	26	51	55	52	·10	34	Cloudy

N.B. The Barometer's height is taken at one o'clock.

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END OF THE FORTY-THIRD VOLUME.

NATURAL HISTORY

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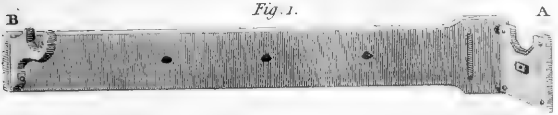
The Englefield Improved Side Transit Instrument, by Thos. Jones, of Oxendon Street.

Fig. 1.



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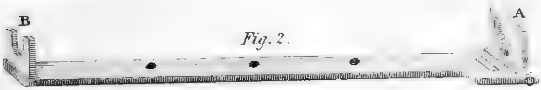

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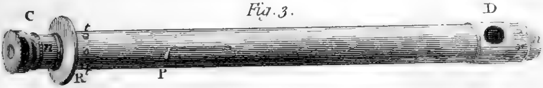

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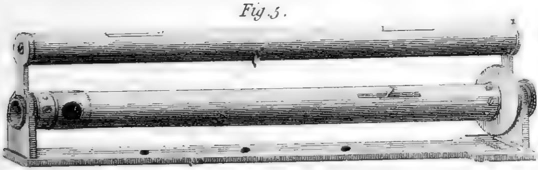

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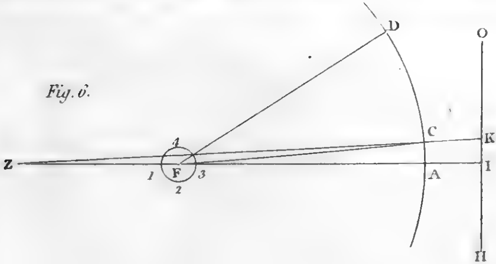

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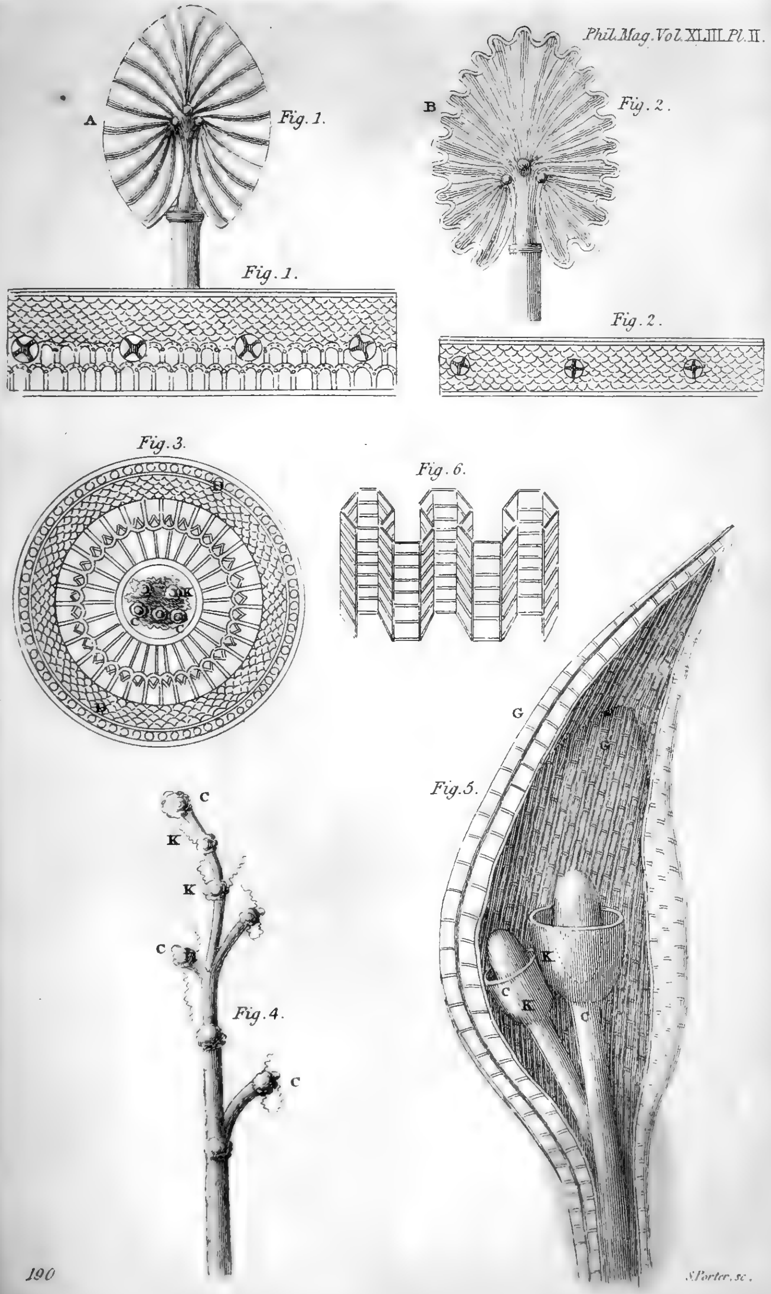

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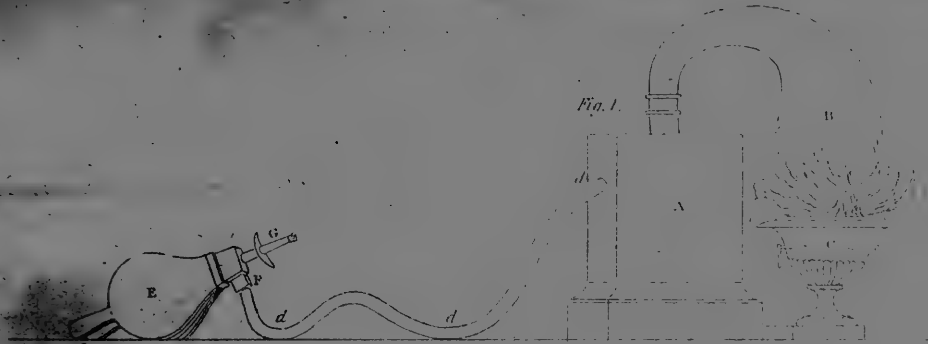

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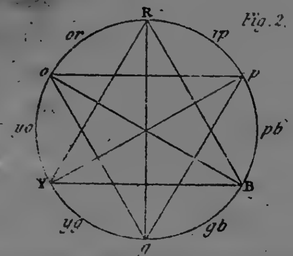

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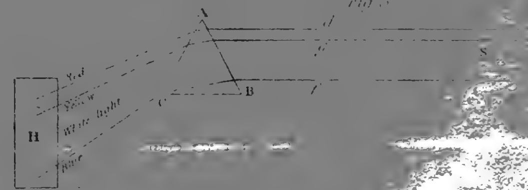
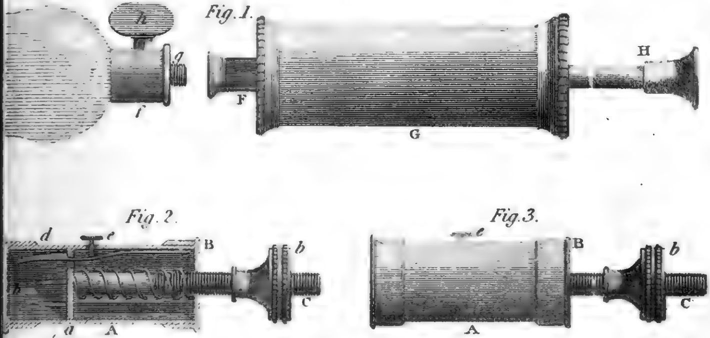


BRITISH MUSEUM

Mr. J. Whitford's Mechanical Substitute for Leeches.



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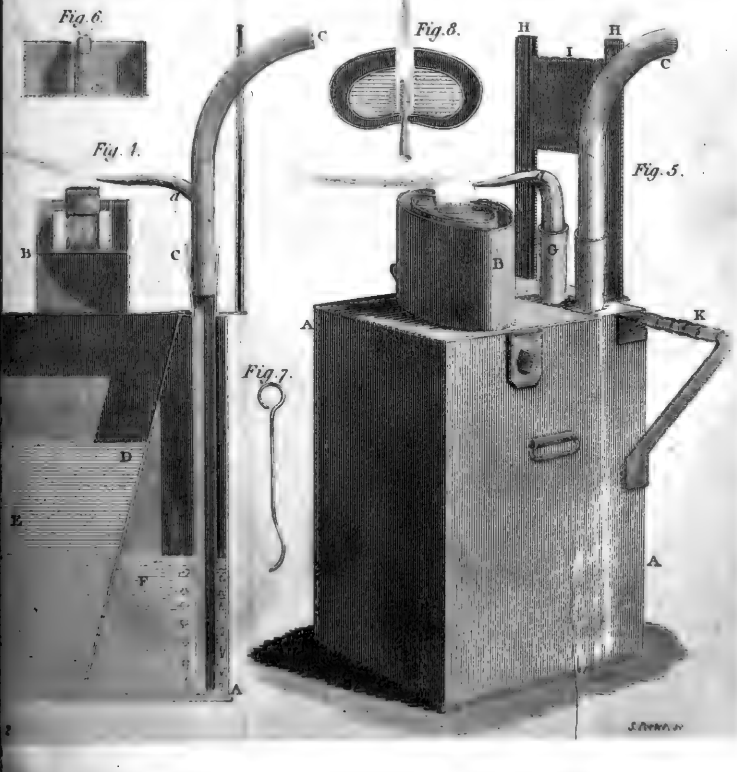

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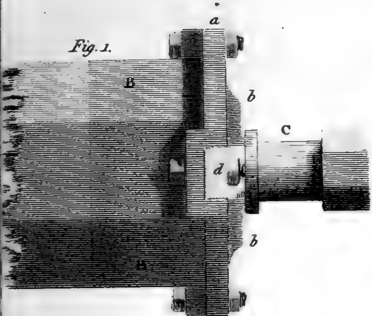

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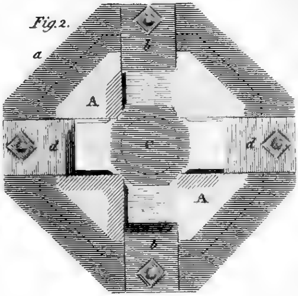

Mr. R. Hughes's Gudgeon, for the Shaft of a Water Wheel.

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Fig. 4.

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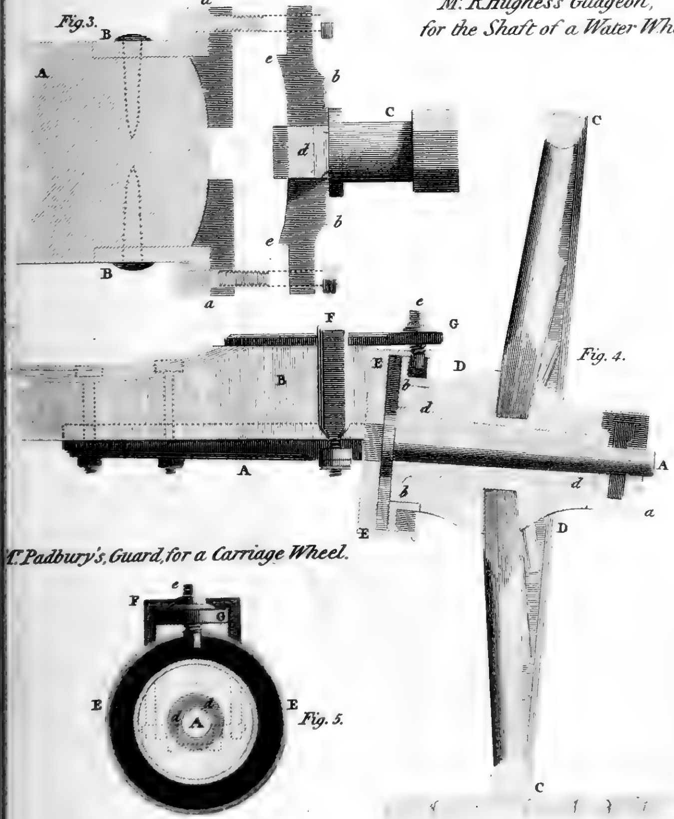

Mr. Padbury's, Guard, for a Carriage Wheel.

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Captn. Peats temporary Ships Rudder.

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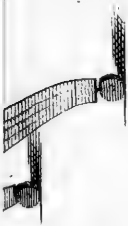

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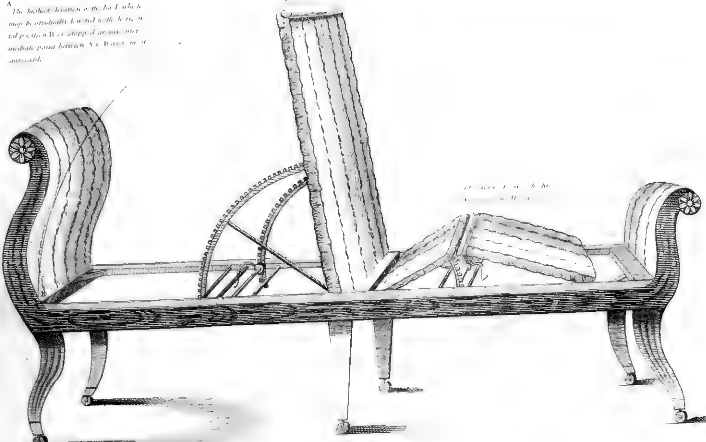

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ADVERTISEMENT.

NEARLY seventeen years have elapsed since the PHILOSOPHICAL JOURNAL was commenced by Mr. NICHOLSON, and sixteen since the appearance of the first Number of the PHILOSOPHICAL MAGAZINE.

During this period the sciences and arts have made the most rapid progress. Numerous Philosophical and Mechanical Instruments and Machines have been invented and improved:—the theory and practice of Astronomy has been greatly advanced:—new Planets have been discovered, and the structure of the Sun more clearly ascertained. The Rays of Light have been subjected to new experiments, which have demonstrated their separate and distinct powers of illuminating and of heating; and that wonderful property upon which the phænomena of the Island Crystal depend, but which is now known to be inherent in both kinds of rays, and universally operative in all the cases of Reflection and Refraction. Chemistry has a second time, within the above period, become a new Science—by the discovery of the effects of Galvanism upon its processes, and the development of elementary substances of higher simplicity than were before known;—by the definite proportions of the component parts of bodies;—by the laws of Elective Attractions and of crystalline forms;—and, even to a certain extent, by a discovery of the remoter causes of those laws and forms. The cultivators of the Sciences, the directors of the operations of the Arts, with public Institutions and Societies, have become every year more numerous and rapid in their increase; at the same time that new Roads, Bridges, Canals, and other National Works, as well as private undertakings, have been every where established to an unprecedented extent.

Under such circumstances of national vigour and exertion, we have found the situation of Journalists of the Discoveries and Improvements made by Philosophers and Men of Research, in every part of the civilized world, to be most honourable and truly gratifying. Public approbation, private friendship, and a reasonable remuneration, have constituted our reward. Upon various occasions, however, our Correspondents have complained that the same Academical Papers, and Articles of Information, have been printed in both Journals; and they have, in their separate letters, requested us to consult together upon some means of remedying this inconvenience, and increased expense upon many of our Readers. But it seemed impossible to obviate this inconvenience in two distinct works; both equally under engagements to present to the Public every thing that might prove new, interesting, and valuable. We have conferred upon this and other considerations which might render our intercourse with the public more effectually useful; and the result of our deliberations has been, that it would certainly be best that we should unite, and that the joint product of our exertions and our correspondence should be consolidated in one Periodical Work; affording all that has hitherto been considered as desirable in the plans and conduct of both; but free from the objection just stated, and perhaps one or two more upon which it is needless to enlarge.

The Philosophical Journal will henceforth be discontinued; and *The Philosophical Magazine* will be conducted by WILLIAM NICHOLSON and ALEXANDER TILLOCH, in the same manner as it has always been carried on; but with every attention to improvement which the joint exertions of the Editors, and the Communications of their Friends and Correspondents can afford.

Mr. NICHOLSON, therefore, requests from his Readers a continuance of that intercourse with which he has so long been gratified; and that the patrons of the *Philosophical Journal* will give their orders for the *Philosophical Magazine and Journal*, through the medium of their own Booksellers, as usual. Communications addressed to the Editors, Pickett Place, Temple Bar, will meet with due attention.

London, Dec. 31, 1813.